Popular Astronomy

Vol. LV, No. 2 FEBRUARY, 1947 Whole No. 542

The Seventy-Sixth Meeting of the American Astronomical Society

By C. M. HUFFER

The 76th meeting of the American Astronomical Society was held in Cambridge, Massachusetts, from December 27 to 30, 1946, inclusive. This meeting was held in conjunction with the American Association for the Advancement of Science and was of a special nature, because it included a celebration of the 100th anniversary of the establishment of the Harvard College Observatory on its present site. At the opening session the director, Dr. Harlow Shapley, stated that the centenary celebrated the 100th anniversary of the first salaries paid to Harvard astronomers and the first observatory deficit.

As is evident from the picture, this was a large meeting. The official record book contains 259 signatures, but these include some duplicates. (Our Russian guests signed each time the book was circulated.) The Council meeting broke the all-time record for attendance. Only three members of the Council failed to attend. All three are West-Coast representatives and one of them had been expected but was still in the hospital after a serious accident.

Because of the centennial celebration, a special kind of program had been arranged. Four symposia—no less—composed the major sessions. The subjects for the invited papers were along four different lines of modern astronomical, perhaps we should say astrophysical, research. All were reports of progress and gave hints of lines of research to be expected in the future. The four symposia were based on the following major subjects:

1. Interstellar matter.
2. Electronic and computational devices.
3. Eclipsing binaries.
4. The gaseous envelope of the earth.

There was one additional session for contributed papers on miscellaneous subjects. All the symposium papers will be published in a special centennial memorial volume. Abstracts of the contributed papers will be published in *The Astronomical Journal* as usual.

This meeting also made astronomical history by beginning what is hoped will be a long series of special lectures dedicated to Professor Henry Norris Russell. Professor Russell himself gave the first lecture.

It was thrilling to participate in the ovation given to the Dean of American astronomers. The lecture traced the development of astronomical knowledge obtained from a study of eclipsing binaries, a favorite field of research by Dr. Russell for half of a century. Work on the theory of eclipsing binaries is still very much alive and Professor Russell is an active leader in new developments of theory and methods of calculation.

The Russell lectures are to be financed by a special fund which has been provided by contributions from several hundred of Professor Russell's friends. The amount of the fund was $9820 at the time of the Council meeting the night before the first lecture. Through the generosity of two members of the Society, the presiding officer was able to announce that the amount was exactly $10,000 at the time of the lecture. The Harvard Observatory gave the amount necessary to finance the first lecture and the Society provided a beautifully illuminated booklet dedicating the series of lectures to Professor Russell and a scroll listing the donors of the fund.

The director of the Harvard Observatory also played an important part in the program. On the first afternoon, December 27, a portrait of Dr. Shapley was presented to Harvard University by his former students and associates. This gift commemorated the 25th anniversary of Dr. Shapley's directorship of the Observatory. Dr. Leo Goldberg introduced the artist, Mr. Charles Hopkinson, who unveiled the portrait. Dr. Goldberg then made the presentation and the portrait was accepted for the University by President Conant, who was also the President of the A.A.A.S. Dr. Conant spoke of Dr. Shapley as a meteor who flies around but not off the earth. It apparently was quite a feat to catch the subject at his desk long enough to paint his portrait.

The Secretary witnessed first-hand one of the meteoric flights of Dr. Shapley. After the end of the meeting, the Secretary visited the pressroom of the A.A.A.S. in the Statler Hotel. As he entered the room the phone was ringing and a press secretary asked, "Where's Dr. Shapley?" Dr. Shapley presumably answered the telephone. Ten minutes later in the subway at the Park Street transfer point, Dr. Shapley was darting through the crowds hurrying to catch a Cambridge train. That night he left by plane for India.

The Saturday morning session was held in the computation laboratory which houses the great Harvard computing machine. After the three papers on the symposium, the 200 people present were divided into four groups and taken to see the machine in operation. Guides were in attendance to explain the operation, but it was impossible to appreciate the intricate machinery and we had to be satisfied with a short explanation. It was possible, however, to see the wheels turning out work in a few seconds which by the usual methods of computation would take hours.

The tea at the Shapley home on Saturday afternoon was one of two special social events. This tea was for those attending the Society meeting only. Everybody was there. In the receiving line were the Shapleys, the Boks, and the Menzels—the director and assistant directors of the Observatory and their wives. The informal discussions of astronomical matters begun at the formal sessions were continued while the astronomers enjoyed the delicious food provided by Mrs. Shapley.

The second social event of the meeting was the special dinner at the Continental Hotel on Sunday evening, followed by a joint meeting with Sigma Xi at the Sanders Theater, where Dr. Shapley gave his lecture as retiring President of the American Astronomical Society and Christmas lecturer of Sigma Xi. The weather, which had been variable but not too disagreeable, was really bad Sunday night. About six inches of snow had fallen the night before, but the snow turned to rain Sunday afternoon. But in spite of the difficulty of walking in the icy rain from the Observatory to the Continental, more than 200 people were on time for the dinner.

After dinner President Struve announced the election of six honorary foreign members of the Society. Usually one honorary member is elected each year. However, during the war years only two foreign members were elected. This year five were elected to fill the vacancies plus one for the year 1946. Those elected were:

Dr. Megh Nad Saha, Calcutta, India.
Prof. Gavrill A. Tikhov, Russia.
Prof. Bertil Lindblad, Upsala, Sweden.
Dr. Jan H. Oort, Leyden, Netherlands.
Prof. E. A. Milne, Oxford, England.
Dr. Bernard Lyot, France.

Dr. Struve next presented the Annie J. Cannon prize to Mrs. Emma Williams Vyssotsky. This prize is presented every three years to an outstanding woman astronomer. It consists of a medal and a check from the proceeds of a fund bequeathed for the purpose by Miss Cannon. The 1946 award is the fifth. Because of ill-health Mrs. Vyssotsky was unable to be present to receive the prize. Her husband accepted the award for her and expressed thanks for Mrs. Vyssotsky and his own approval of the selection by the Council.

The President then called on one of the Russian guests present at the dinner—Dr. G. Shajn of the Semeis Observatory in the Crimea, U.S.S.R. Dr. Shajn thanked the Society for the election of Prof. Tikhof, the Senior Russian astronomer, to honorary membership. He also spoke of the improvement in international cooperation in scientific research since the war and ended by saying: "We have now a problem of cultivation of the science of good human relationships, the ability of

all peoples of all kinds to live and to work together in the same world of peace."

The dinner ended with a resolution of thanks on behalf of the Society written and presented by Dr. Joel Stebbins, one of the two past presidents on the Council. The resolution in full is as follows:

Mr. President and Friends:

When one has had a fine time at a party it is customary to say a word of appreciation to the host and hostess and then make a reasonably quick and graceful exit. This time, some of us have thought that we might perform this pleasant duty at the session which was sure to have the largest attendance. Let me hasten to add that no significance should be given to the fact that the vote of thanks I am going to propose will be taken before the address later this evening.

The first meeting of the Society at the Harvard Observatory was in the time of Professor Edward C. Pickering, whose forty years as director of the Observatory and fifteen years as president of the Society left a permanent, inspiring influence on both institutions. This now is about the tenth meeting of the Society at Harvard. (Ten is at least the right order of magnitude.) We might have a standard form of appreciation and simply fill in the proper date, but somehow each meeting seems better than the one before.

The Society was founded at the Yerkes Observatory, but if the Yerkes Observatory can be called the father, surely the Harvard Observatory is the mother of the Society. Our mother is now 100 years old; the Society will soon be fifty; and only the other day we celebrated the 25th anniversary of a promising grandchild. The Society will remain perpetually youthful; the centenarian grandmother shows no signs of decrepitude; and the grandchild remains perennially the same. Incidentally, the conception and organization of the Russell Lectures was no mean achievement for an inexperienced youngster.

We are at a loss where to begin or end our thanks. The Harvard calculator is a great machine, but we ought to be able to feed into it simply the names of President Conant, Dr. and Mrs. Shapley, the Boks, and the Menzels, and have it grind out and deliver an engraved card of thanks to each member of the Observatory staff, with proper division of the awards of merit, not forgetting the operator of the machine itself. But if the machine should fail we hope that the record of our appreciation will trickle through to the right places, including Oak Ridge and Radcliffe.

Someone has said that if you get within twenty miles of Shapley or Struve you are in for a symposium, but here, with this pair of kindred spirits working together, we got not two, but two times two symposia. To those who feared that four symposia would make this a meeting to end all meetings, we can say that while it has been a severe ordeal a few survivors will be left, gasping for more.

Again we congratulate the Harvard Observatory on the completion of its first 100 years, and we trust that your satisfaction will not be tempered by the warning that we expect to be back not ten but twenty times in the century to come.

Mr. President: If the present members of the Observatory staff and their families will kindly remain seated so that at long last we can see who they are and count them, I would move a rising vote of thanks for a splendid meeting.

Following is the list of names of persons who were elected as members of the American Astronomical Society at the 75th meeting, September, 1946.

Mr. Custer C. Baum, Hughes Aircraft Co., Culver City, California.
Mr. William Blitzstein, Philadelphia 31, Pennsylvania.
Miss Simone G. Daro, Harvard College Observatory, Cambridge 38, Massachusetts.
Mr. Armin J. Deutsch, Yerkes Observatory, Williams Bay, Wisconsin.
Professor Floyd F. Helton, Central College, Fayette, Missouri.
Rev. Roger Leclaire, Harvard College Observatory, Cambridge 38, Massachusetts.
Dr. Harald H. Nielson, Ohio State University, Columbus 10, Ohio.
Miss Ruth J. Northcott, David Dunlap Observatory, Richmond Hill, Ontario, Canada.
Mr. James B. Rothschild, Wharton, N. J.
Mr. George R. Schaefer, Washington 17, D. C.
Dr. Dave H. Shaffer, Ohio State University, Columbus 10, Ohio.
Mrs. Martha B. Shapley, Harvard College Observatory, Cambridge 38, Massachusetts.
Miss Elsa van Dien, Harvard College Observatory, Cambridge 38, Massachusetts.

Following is the list of names of persons who were elected as members of the American Astronomical Society at the 76th meeting, December, 1946.

Miss Barbara Bell, Harvard College Observatory, Cambridge 38, Massachusetts.
Professor G. Howard Carragan, Rensselaer Polytechnic Institute, Troy, N. Y.
Dr. Y. C. Chang, Yerkes Observatory, Williams Bay, Wisconsin.
Mr. Bertram D. Donn, Harvard College Observatory, Cambridge 38, Massachusetts.
Dr. Henry F. Donner, Western Reserve University, Cleveland 6, Ohio.
Professor E. R. Dyer, Leander McCormick Observatory, University of Virginia, Charlottesville, Virginia.
Mr. Salah (El-din) Hamid, Harvard College Observatory, Cambridge 38, Massachusetts.
Mr. Joost H. A. Kiewiet de Jonge, Harvard College Observatory, Cambridge 38, Massachusetts.
Mr. Ivan Robert King, Harvard College Observatory, Cambridge 38, Massachusetts.
Mr. Myron G. H. Ligda, 26 Florence Avenue, Arlington Heights, Massachusetts.
Mr. Albert Paul Linnell, Harvard College Observatory, Cambridge 38, Massachusetts.
Mr. Lester C. Mills, Harvard College Observatory, Cambridge 38, Massachusetts.

Mr. Alan H. Shapley, Harvard College Observatory, Cambridge 38, Massachusetts.
Miss Julena Steinheider, U. S. Naval Observatory, Washington 25, D. C.
Mr. John Streeter, Harvard College Observatory, Cambridge 38, Massachusetts.

WASHBURN OBSERVATORY, JANUARY 17, 1947.

The Origin of Twenty-Eight Mansions In Astronomy

By COCHING CHU

INTRODUCTION

From the time of high antiquity Chinese people have practiced agriculture. To these primitive folks an elementary knowledge of astronomy is indispensable; being without a calendar, they had to watch stars in the night in order to determine the proper time for planting of crops, repairing of buildings, etc. The starry night of northwest China, which is the cradle of the Chinese civilization, also favors such a procedure. For an illustration we may cite portions of two Odes from the "Book of Poetry," which is a collection of folklore of early Chow dynasty, said to have been edited by Confucius himself. In the Odes of Pin stand these verses:[1]

> "In the seventh month, the Fire star passes the Meridian,
> In the 9th month, clothes are given out,
> In the days of first month, the wind blows cold,
> In the days of second, the air is cold;—
> Without the clothes and garments of hair,
> How could we get to the end of the year?
> In the days of third month, they take plough in hand,
> In the days of fourth, they take their way to the field,
> Along with my wife and children,
> I carry food to them in those southlying acres,
> The surveyor of the field comes, and is glad."

Again in the Odes of Yung come these verses:[2]

> "When Ting culminated at night fall,
> He began to build the palace at Tsoo.
> Determining its aspects by means of the sun,
> He built the mansion at Tsoo."

Now "Fire star" is the early Chinese designation for the red star Antares, in the constellation Scorpio; and "Ting" signifies Pegasus square, which culminated at night fall in the autumn months during Chow dynasty. In the "Book of Poetry," which has been universally acknowledged by the Chinese scholars as the most authentic of the Chinese classics, besides the mentioning of Fire star and Ting, the following stars or asterisms also appeared: Mau or Pleiades, Pi or Taurus, Ki or Sagittarius, Chien Nieou or Aquila, Tse-Niu or Lyra, and Tsan or the belt of Orion. These constitute the nucleus of the 28 Mansions

of China. A complete list of 28 Mansions, however, did not appear until a much later date, about 200-500 B.C., in any of the four following books: Li-ki, Er-yah, Wei-nan-tze, and Lu-shi-chun-chiu.

The 28 Mansions or Siu or Chinese astronomy were made known to the western world in the eighteenth century by the Catholic Father, P. Gaubil, whose book "Chronologie Chinoise" was well known to astronomers of that time. In 1807, Colebrook with the help of Hindu Pundits, identified the principal stars or Jogotaras of 27 or 28 Nakshatras of India, the longitudes and latitudes of these principal stars being given in several Indian works, the Surya and Brahma Siddhantas, the Siddhanta Siromani, and the Grahalagava.[3] A comparison of Chinese Siu with the Hindu Nakshatra makes it plain that they are common in origin. For the last hundred years the question whether the 28 Mansions were originated in China or in India has been a mooted problem. Two French scholars, J. R. Biot in the middle of the 19th century, and Leopold de Saussure in the first part of the 20th, Gustav Schlegel of Holland, and the late Professor S. Shinzo of Japan were all in favor of Chinese origin; while the German scholar, L. Weber, the American Indianist, W. D. Whitney of Yale University, the Rev. J. Edkins of *China Review,* and the author of "Hindu Astronomy," W. Brenand, all held contrary views. With the end of the Second World War and with the reopening of the Burma Road and coastwise communications, Sino-Hindustanian relations should be much closer than heretofore, commercially as well as culturally. It is interesting, therefore, to bring up a question which reminds one of intellectual cooperation and borrowings of these two neighboring peoples millenniums ago.

A. Common Origin of Hindu Nakshatra and Chinese Siu

The common origin of Hindu Nakshatra and Chinese Siu is not doubted. A glance at the tables I and II will show that in 9 of the 28 Mansions, *i.e.,* Kio, Ti, Tche, Py, Leou, Wei, Mao, Tseu, and Tchin, the Jogotaras of the Hindu system are identical with the controlling stars of the Chinese system, while in 11 of the rest, even though the determining or controlling stars are not identical, they belong to the same Western constellation. Only eight asterisms have Jogotaras of Hindu system and controlling stars of Chinese system in entirely different contellations. Of these eight it is interesting to note that for the asterism Niu or "woman," the Hindus used α Aquilae instead of ϵ Aquarii, and for the asterism Nieou or "ox," the Hindus used α Lyrae instead of β Capricorni. Now in China α Lyrae is called Tse-Niu, or "a weaving woman" and α Aquilae is called Chien-Nieou, or "leading an ox to sacrifice." It is most probable that in ancient times the Chinese, like the Hindus, also used α Aquilae and α Lyrae as leading stars in their 28 Mansions instead of ϵ Aquarii and β Capricorni which are comparatively inconspicuous stars. This was made evident

by the fact that the latter two stars were not cited in the "Book of Poetry," while mentions were made of the former in the same book.[4] In Li-ki as well as in Erh-yah, Wei-nan-tze, and Lu-shi-chun-chiu, Niu and Nieou were already used in the place of Tse-Niu and Chien-Nieou. The change, therefore, must have been made in the intervening years. With the advance of knowledge of observation, such alteration is but natural, for both Vega and Altair, are too far from the ecliptic, and therefore not convenient for the location of the moon or the sun. In "Uranographie Chinoise," Gustav Schlegel stated that in the planisphere of Denderah in the temple of Hother, Egypt, both the "woman" and "a man accompanied by a cow" were represented in the zodiac of Capricornus, and in the 3rd decan of the Hindu sphere of Aben-Ezra, a fish and a woman weaving cloth were depicted.[5] All in all, the agreement of Hindu Nakshatra and Chinese Siu is so remarkable that we must conclude that they had the same origin. Kumagusu Minakata, a Japanese scholar, maintained that "the Chinese records of the typical con-

TABLE I

THE TWENTY-EIGHT MANSIONS OF CHINA

No.	Chinese Siu	Meaning	No. of Stars	Controlling Star: Greek name	Mag.	R.A. (1900) h m s	Decl. (1900) ° ′ ″
1	**Kio**	Horn	2	α Virgo	0.9	13 19 55	−10 38 22
2	**Kang**	Neck	4	κ Virgo	4.2	14 07 34	− 9 48 30
3	**Ti**	Breast	4	α^3 Libra	2.8	14 45 21	−15 37 35
4	**Fang**	Room	4	π Scorpio	2.9	15 52 48	−25 49 35
5	**Sin**	Heart	3	σ Scorpio	3.0	16 15 07	−25 21 10
6	**Quei**	Tail	9	μ Scorpio	3.1	16 45 06	−37 52 33
7	**Ki**	Winnowing tray	4	γ Sagittarius	2.8	17 59 23	−30 25 31
8	**Teou**	Bushel	6	φ Sagittarius	3.2	18 39 25	−27 05 37
9	**Neou**	Ox	6	β Capricornus	3.2	20 15 24	−15 05 50
10	**Niu**	Woman	4	ε Aquarius	3.8	20 42 16	− 9 51 43
11	**Hiu**	Emptiness	2	β Aquarius	2.9	21 26 18	− 6 00 40
12	**Koey**	Covered house	3	α Aquarius	3.9	22 00 39	− 0 48 21
13	**Tche**	House	2	α Pegasus	2.6	22 59 47	+14 40 02
14	**Py**	Wall	2	γ Pegasus	2.9	0 08 05	+14 37 39
15	**Koei**	Sandal	16	η Andromeda	4.2	0 42 02	+23 43 23
16	**Leou**	Basket for harvest	3	β Aries	2.7	1 49 07	+20 19 09
17	**Wei**	Stomach	3	41 Aries	3.1	2 44 06	+26 50 54
18	**Mao**	Setting sun	7	η Taurus	2.8	3 41 32	+23 47 45
19	**Pi**	Net	8	ε Taurus	3.6	4 22 47	+18 57 31
20	**Tseu**	Lips	3	λ′ Orion	3.4	5 29 38	+ 9 52 02
21	**Tsan**	3 stars	10	ζ Orion	1.7	5 35 43	− 1 59 44
22	**Tsing**	Well	8	μ Gemini	3.0	6 16 55	+22 33 54
23	**Koei**	Ghost	4	θ Cancer	5.8	8 25 54	+18 25 57
24	**Lieou**	Willow	8	δ Hydra	4.2	8 32 22	+ 6 03 09
25	**Sing**	Neck of a bird	7	α Hydra	2.0	9 22 40	− 8 13 30
26	**Tchang**	Bill of a bird	6	μ Hydra	4.0	10 21 15	−16 19 33
27	**Yi**	Wings of a bird	22	α Crater	4.2	10 54 54	−17 45 59
28	**Tchin**	Carriage	4	γ Corvus	2.6	12 10 40	−16 59 12

N. B. The names of controlling stars are taken from G. Schlegel's "Uranographie Chinoise." The right ascensions and declinations are taken from L. Boss, "Catalogue of 6188 stars for the epoch 1900," Carnegie Institution, Washington, D. C.

stellations dated further back than the epoch of their intercourse with the Indians. The Indian constellations . . . are essentially of Brahmanical type and thus proclaim their priority in existence to the event of the Buddhist mission to China, which marks the era of the mutual

TABLE II

THE HINDU NAKSHATRAS

No.	Nakshatra	Meaning	No. of stars in Nakshatra	Jogotaras — Greek Name	Mag.	R.A. (1900) h m s	Decl. (1900) ° ′ ″
1	Aswini	A horse's head	3	β Aries	2.7	1 49 07	+20 19 09
2	Bharani	Youi	3	41 Aries	3.5	2 44 06	+26 50 54
3	Kritics	A razor	6	η Taurus	2.8	3 41 32	+23 47 45
4	Rohini	A wheel carriage	5	α Taurus	0.9	4 30 11	+16 18 30
5	Mriga	The head of an antelope	3	λ′ Orion	3.4	5 29 38	+ 9 52 02
6	Ardra	A gem	1	α Orion	1.0	5 49 45	+ 7 23 18
7	Punarvasu	A house	4	β Gemini	1.1	7 39 12	+28 16 04
8	Pushya	An arrow	3	δ Cancer	4.1	8 39 00	+18 31 19
9	Aslesha	A wheel	5	ε Hydra	3.4	8 41 29	+ 6 47 09
10	Magha	Another house	5	α Leo	1.2	10 03 03	+12 27 21
11	Purva Phalguni	A bedstead	2	δ Leo	2.5	11 08 47	+21 04 18
12	Uttra Phalguni	Another bedstead	2	β Leo	2.2	11 43 58	+15 07 52
13	Hasta	A hand	5	γ Corvus	2.6	12 10 40	—16 59 12
14	Chitra	A pearl	1	α Virgo	0.9	13 19 55	—10 38 22
15	Swati	A piece of coral	1	α Bootes	0.0	14 11 06	+19 42 10
16	Visakha	A festoon of leaves	4	α Libra	2.8	14 45 21	—15 37 35
17	Anuradha	An obligation to the gods	4	δ Scorpio	2.3	15 54 25	—22 20 14
18	Jyeshtha	A rich earring	3	α Scorpio	0.8	16 23 17	—26 12 37
19	Mula	The tail of a fierce lion	11	λ Scorpio	1.5	17 26 49	—37 01 58
20	Purva-shadha	A couch	2	δ Sagittarius	2.7	18 14 36	—29 52 14
21	Uttar-shadha	The tooth of a wanton elephant, near which is the kernel of the sringataca nut	2	τ Sagittarius	3.3	19 00 42	—27 49 00
22	Abhijit	A grain of rye	3	α Lyra	0.0	18 33 33	+38 23 05
23	Sravana	The three footed step of Vishnu	3	α Aquila	0.6	19 45 54	+ 8 36 15
24	Dhanishta	A tabor	4	α Delphini	3.9	20 35 00	+15 33 33
25	Satabhisha	A circular jewel	100	λ Aquarius	3.8	22 47 24	— 8 06 43
26	Purva Bhadra-pada	A two-faced image	2	α Pegasus	2.6	22 59 47	+14 40 01
27	Uttara Bhadra-pada	Another couch	2	γ Pegasus	2.9	0 08 05	+14 37 39
28	Revati	A smaller sort of tabor	12	ζ Piscium	5.5	1 08 30	+ 7 02 47

N.B. The Hindu star names and their Greek identities are taken from W. Brenand "Hindu Astronomy."

acquaintance of two nations."[6] Hence he argued that though the two systems are practically coincident, they had separate origins. Thanks to the researches of sinologues for the last 20 or 30 years, we now know that the intercourse between Chinese and Hindus far preceded in order of time the Buddhist mission in the later Han dynasty (67 A.D.), and astronomical evidence may antedate the sinological discoveries still further.[7]

B. Where was the System of 28 Mansions Originated?

If we admit that the Chinese Siu and the Hindu Nakshatra had the same origin, then the question arises as to where was this Siu or Nakshatra invented. Since 1839, when a series of articles by J. B. Biot began to appear in *Le Journal des Savants*, and when L. Ideler published his book "Über die Zeitrechnung der Chinesen," a controversy waged hot between the Indianists on the one hand and the Chinophiles on the other. As is usually the case in such a strife, much heat was evolved but little light was shown at first. There are three hypotheses in connection with the communal origin of the 28 Mansions: (1) The system was originated in China, (2) it was invented in India, and (3) both the Chinese and Hindus borrowed it from Babylonia or some other country in western Asia. Each hypothesis has its staunch supporters. The French savant, Leopold de Saussure, made an admirable résumé of the pros and cons of the arguments advanced by different authors in his first two papers on Chinese astronomy, published in T'oung-Pao.[8] The difficulty of settling the question lies in the fact that no authentic record of Chinese history existed earlier than Chow dynasty (1122-246 B.C.): the recent unearthing of divination bones and tortoise shells of the later Shang dynasty at An-Yang, dated Chinese history back 300-400 years earlier to 1400 B.C. But beyond that, Chinese history has to depend upon the conjectures and interpretations of later writers, who endeavored to give meanings to passages contained in the old classics, which had reference to doings of early Emperors like Yao and Shun (*circa* 2400 B.C.). The situation in India is not much better. The first evidence of great antiquity of Hindu astronomy was made known to the western world by the publication of certain astronomical tables in the "Memoir of the French Academy of Sciences" in 1687. In the later part of the 18th century, a Frenchman, M. Bailly, the author of "Traité de l'Astronomie Indienne," maintained that a general conjunction of the sun, moon, and planets was observed by the Hindu astronomers in the year 3102 B.C. This statement was much controverted at that time, and opinion was advanced that the epoch of 3102 B.C. was adopted by the Hindus at a comparatively recent date, only from calculation. No less an authority than the famous Laplace denied its authenticity.[9]

Bearing these facts in mind, we will see that much of the hot discussion on this controversial topic is not fruitful, for many authors based

their conclusions on wrong premises. Both L. Weber and W. H. Whitney believed that no authentic Chinese document existed previous to the third century B.C., for the first Emperor of Tsin dynasty (246-210 B.C.) burned every fragment of the written records then extant. This, of course, is not strictly true. Besides, the burning of the books was not all inclusive, those on medicine and divination were conspicuously exempted. On the other hand, the Chinophiles, especially J. B. Biot and Gustav Schlegel, put too much faith in the authenticity of all Chinese written records. Even Leopold de Saussure[10] was rather credulous at times. According to the recent researches of Chinese scholars, the book Hsia-Siao-Cheng and the text of Yao-Tien in the "Book of History" (Shu-King) were not written at the mythical age of Emperor Yao and Hsia dynasty (2400-1800 B.C.), but were probably composed in the early Chow dynasty.[11] The books Chow-Li, as well as Chow-Pi, in which the principle of determining the date of solstices by means of measuring the shadow of a gnomon was mentioned, could not be the work of Chow-Kung, the brother of the first Emperor of Chow dynasty. The Chow-Li was written in the period of 500-300 B.C. and Chow-Pi not until the later Han dynasty.[12]

In discussing the system of 28 Mansions of China and India, three cardinal facts must be firmly grasped: (a) While Babylonians, Egyptians, and Greeks observed the solar position in the zodiac by watching heliacal rising of stars, the Chinese and Hindus, from the earliest times, determined the sidereal place of the sun by noting that of full moon. The advantage of this latter procedure is obvious. This fact was clearly stated by de Saussure. He said: "When the moon is full she is exactly diametrically opposite the sun. As the sun comes back every year, in the same month, into the same constellation (then invisible) it follows that the full moon happens every year in the same month in the opposite constellation. . . The observations of moon's 'full' . . . does not necessitate the use of any artificial point of reference, and the precision of its result is much superior to that which might proceed from the observation of the star's rising. These risings vary with the same slowness as the annual course of the sun, namely, one degree per day. Besides, the mists of the horizon and the variation in the atmospheric state seldom allow us to distinguish the stars when their height is small, so that it is often difficult to fix, within 10 days, the one whose heliacal rising immediately precedes dawn, or the one whose acronychal rising has taken place opposite the setting sun.

"Such is not the case when observing the sidereal place of the moon, if one knows exactly the time of her 'full.' In observing the aspect of the disc one could no doubt commit a great error: for it is impossible at first sight to discern the 'full' within a day. But some ancient Chinese traditions show us they could appreciate the exact 'full' within a quarter of a day, a decided proof that they made use, for this determination,

of the simultaneousness of the moon's rising with sunset. . . During the primitive period, when the year's course was still only indicated by the sidereal points of reference, this process of observation permitted the fixing of an annual date and the rectifying of the lunar year. It suggested the opportunity of adding an intercalary month. In China, it was Kio which served this end. That star, of first magnitude, marked in the firmament the entrance of the Spring Palace. The full moon which showed to the right of Kio was the 12th (or 13th) of the year and the one which showed to left of Kio was the first of the new year."[13]

According to Professor P. C. Senguta of Calcutta University, during the Brahman period all the lunar months were named according to the Nakshatra in which the full moon happened to occur. When the full moon took place in the Nakshatra "Phalguna" the very next day shall be the new year's day and the first month of the year is called "Phalguna." In still earlier times, spring began one day after the moon in Chaitra, which is equivalent to Chinese Mansion Kio.[14] This similarity of reckoning the new year in India and in China again amply proves the identity of the two systems.

(b) The system of 28 Mansions in China is primarily devised to regulate an agricultural calendar, solar-lunar in character. From the earliest times of the Shang and Chow dynasties, as was made evident by the discoveries in An-Yang and by the various Odes in "Book of Poetry," raising of farm crops had been the main care of most of the population, while hunting and pasturing were mere subsidiary professions. It is doubtful whether the Chinese ever had been nomadic; for no stars or constellations were named after a ram or a goat, and the early Chinese seemed never to have acquired the habit of milk drinking. To these primitive farmers of ancient times, the knowledge of coming of spring or growing season was a vital necessity; especially in northwest China, where winter is long and rigorous. During the second to third millennium before the Christian era, the acronychal rising of the star Antares (α Scorpii), the Chinese Fire star, or Sin, about the time of spring equinox, was made a great occasion. A special officer was created to watch the appearance of this star on the eastern horizon. According to Se-Ma Tsien[15] the downfall of Emperor Sao-Haw (*circa* 2598 B.C.) was brought about by famines, calamities, and invasions of barbarians, mainly due to the confusion of seasons; and his successor, Tsan-Shu, restored the order by establishing two officers; one to observe the culmination of stars on the meridian, and the other to watch the acronychal rising of Fire star or Antares. In Tso-Chuan, it was stated, "the Fire star first appeared in third month during Hsia dynasty, fourth month in Shang dynasty and fifth month in Chow dynasty."[16] All these go to prove that the ceremony of observing the first acronychal rising of Fire star in China is of long standing, and it served

an urgent need of a farming population.

(c) The Chinese new year, from time immemorial began not from vernal equinox but from a point called Li-Chun lying just midway between the winter solstice and vernal equinox.[17] Se-Ma Tsien was again our authority on this statement.[18] The choice of Li-Chun as the beginning of the year is of great significance astronomically. To the early Chinese, three constellations in the sky are in a rank by themselves. These are Scorpio, Orion, and Ursa Major. They are called the Ta-Chin or great rulers of the sky. In Kung-Yung-Chuan, it is stated, "What is Ta-Chin? Great Fire star is Ta-Chin, Orion is Ta-Chin, and the North Pole is also Ta-Chin."[19] Hence "North Pole" is also known as North-Chin. Professor Shinzo interpreted "North Pole" as nothing but the seven principal stars of the constellation Ursa Major.[20] In ancient times the body of the Great Bear was much nearer the north pole than it is now, and the tail of the Great Bear seemed to rotate around the north pole like the hand of the clock. According to Se-Ma Tsien, the seasons of the year could be determined by the position of the handle of the "Great Dipper." There is a clear statement of this method of determining the season in the writings of Hoh-Kwan-Tze: "When the tail of the Bear points to the east (at night fall) it is spring to all the world. When the tail of the Bear points to the south it is summer to all the world. When the tail of the Bear points to the west, it is autumn to all the world. When the tail of the Bear points to the north, it is winter to all the world."[21] Now if we extend the handle of this "Northern Bushel" in approximately the same direction for 30°, we will come to a very bright star Arcturus (α Boötis) or Ta-Kio (great Horn) and if we go 30° still farther we come to another first magnitude star Spica (α Virginis) or Kio (Horn), which is the controlling star of the first Mansion of the Chinese system and supposed to be the horn of the dragon. It is most likely that Ta-Kio formerly occupied a Mansion in Chinese Siu as it still does in Hindu Nakshatra. For the same reason as Chien-Nieou and Tse-Niu yielded their places to Nieou and Niu, so did Ta-Kio yield its place to Kio. Thus Se-Ma Tsien said, "The handle of the 'Great Dipper' brought forth the head of the Dragon" which is the beginning of the palace of spring. It is significant that about 3000 B.C. the full moon occurred in the Mansion of Kio about the period Li-Chun.

If we keep these three cardinal facts in mind, we will be in a better position to appreciate the arguments of different writers, and to evaluate their claims. Both L. Weber[22] and W. H. Whitney[23] believed that the Hindu Nakshatra was earlier than the Chinese Siu by more than one thousand years, on account of the fact that the Hindu system began with the Nakshatra Mao, or Pleiades, while the Chinese Siu commenced with Kio. Kingsmill quoted Weber in saying that the Chinese asterism must be of late introduction, not earlier than the second or third cen-

tury B.C. from the fact that the list as usually presented begins with Spica.[24] The miscalculation lies in the fact that both Weber and Whitney assumed that the ancient Chinese began their calendar with the vernal equinox and not with Li-Chun as was shown above. On the other hand, the Chinophile, Gustav Schlegel, assuming that the Chinese system was formed when Antares, the controlling star of Sin, had its heliacal rising on vernal equinox; and Spica, the controlling star of Kio, had its heliacal rising on Li-Chun, came to the conclusion of 14,000 B.C. as the age of the origin of the Chinese 28 Mansions.[25]

If we disregard these extravagant claims and misguided arguments, we can then come face to face with plain facts. The most valid arguments in favor of Chinese origin are advanced by de Saussure and Shinzo. They are the following: (a) The gradual formation of the Chinese system from the two asterisms, Sin (Scorpio) and Tsan (Orion) and the four cardinal culminating stars of Yao-Tien, to the complete list mentioned in Li-Ki and Erh-Yah, can be traced in several stages; while in India, the system was ready made from start, and so far we do not have any inkling of the process of its formation.[26] (b) Biot made the discovery that the controlling stars of Chinese 28 Mansions came in couples located diametrically opposite each other in meridian, and based on an equatorial system. De Saussure later elaborated this scheme. The first couple chosen was the two Grand Rulers or "Ta-Chins" of the sky: Sin and Tsan. This fact was quoted in Tso-Chuan, and must have come from a source of great antiquity.[27] The Hindus, even though they used practically an identical system, seemed never to have noticed this coupling effect. (c) In Vedic astronomy, the Hindu new year commenced with Phalguna. But Professor P. C. Senguta stated that in ancient times in India "Spring began one day after the moon in Chaitra"[28]; from that fact he concludes that the point of summer solstice must have been near δ Leonis about the period 3100 B.C. From Senguta's statement it is clear that the ancient Hindu new year, like the Chinese, should come in the month when the full moon entered the Mansion Kio.[29] But as mentioned above, the Chinese 28 Mansions commenced with Kio, because the ancient Chinese regard the "Great Dipper" as the arbiter of seasons, and the handle of the "Dipper" happened to point to Mansion Kio. In India the Nakshatras do not seem to bear any relation with the "Great Dipper." This fact seems to confirm the contention of Professor Shinzo that the system of 28 Mansions must have been given rise in a region where the constellation Ursa Major had been taken as the standard of time keeper.

Besides the three points mentioned above, there are still two other facts which, in the opinion of the present writer, also merit attention: (d) While ancient Hindus may have excelled the Chinese of the same period in theoretical calculations in the realm of astronomy, they were not as good in making observations. The ancient Chinese observations

on solar eclipses, sunspots, and comets were well known. A catalogue of 120 stars with polar distances and declinations was compiled by two astronomers, Kan and Si. Basing on these data, Professor Shinzo and his colleagues computed the date of cataloging to be 350 B.C., or nearly 200 years earlier than the famous star catalog of Hipparchus and Ptolemy. Even the Indianists and Hindu scholars admit that the ancient Hindus were not at all diligent in making astronomical observations. According to W. Brenand, "The Hindus, unlike the ancient Chinese, had not the ambition of making a catalogue of all stars which were visible to them. They had a more important object in view, namely, the study of the motions of the sun, the moon, and the planets and other astronomical phenomena, primarily for the purpose of computing time and of constructing and perfecting their calendars. Such an object, they knew, could not be materially advanced by ascertaining merely the positions of stars fixed beyond or outside the course of the moving celestial bodies; and they accordingly confined their attention to those stars which lay in moon's path, immediately north or south of the Ecliptic."[30]

Dr. Bhutan Dutta, lecturer in mathematics at Calcutta University, in an article on "Vedic mathematics" also said: "They (the Hindu astronomers) noted very few stars lying outside that belt (Ecliptic). Probably they did not search for others."[31] The fact that the ancient Hindus did not discard such stars of high latitude and great declination, as Vega and Arcturus, from their list of Jogotaras shows that they were not very careful even in observing the stars along the moon's track, for it is not possible for either the moon or the planets to approach anywhere near one of these stars. Besides, without long years of assiduous gazing of sky, it is most improbable that a system of 28 Mansions could be devised. So the natural conclusion would be that the whole system of Nakshatra was imported in a body.

(e) Many authors, including Ideler, the Rev. John Chalmers, and Kingsmill, criticized the Chinese system because of its asymmetry. The 28 Mansions were divided into four quarters of 7 Mansions each; these were called by Se-Ma Tsien, Spring, Summer, Autumn, and Winter Palaces. They vary greatly in length: Spring Palace (east) occupies 70° 50′, Winter Palace (north) 101° 10′, Autumn Palace (west) 75° 40′, and Summer Palace (south) 112° 20′. The angular value of each Mansion also varies greatly from more than 30° to less than a degree. Such a heterogeneous grouping of stars would outrage the aesthetic sense of an artist or the taste for precision of a mathematician. But the ancient Chinese, being highly practical in taste, did not bother with fine adjustment of symmetry, as long as the scheme worked to foretell the coming and going of seasons. Indeed, it is the very nonuniformity of the length of four Palaces that proves the indigeneity of the system of 28 Mansions to China.

As referred to above, the 28 Mansions system in China is nothing but an agricultural calendar, by which the primitive farmers can date their ploughing of land, planting and transplanting of seeds and seedlings, clearing of weeds, harvesting of crops, etc. The division of a year into four seasons in China is very natural; spring for planting, summer for growing, autumn for harvesting, and winter for resting. Northwest China, however, has a continental climate; winters are apt to be severe and summers rather torrid, and both are of long duration, with short spring and autumn in between. In India the year is divided either into three seasons or into six,[32] but not into four, and yet the Nakshatras are divided into four groupings of seven each,[33] which could not serve any useful purpose. It seems, therefore, the exotic nature of 28 Mansions to India is undeniable.

When two great ancient civilizations, like those of China and India meet, the flow of intellectual current cannot be a one way traffic. There are ample proofs that the ancient Chinese had borrowed abundantly from India even before the Buddhist mission to China in the later Han dynasty. In astronomy the cycle of Jupiter in China was distinctly foreign in origin. The twelve names of the cycles first appeared in Erh-Yah and Wei-Nan-Tze; later Se-Ma Tsien employed them in his "Historical Record" to arrange the years of cycles of sixty. The names of the cycle are words of two and three syllables, and singularly exotic in character. They are now rarely used. For the last two thousand years the Chinese scholars from Kuo-Poh, the commentator of Erh-Yah, to Mr. Liang Chi-Chao of recent past, endeavored to discern their meaning and to decipher their origin, but all in vain. The cycle starts with Shitika. The Rev. John Chalmers suspected the term Shitika to be the corrupted version of the Hindu word for Jupiter, "Vrishaspati"; while the Rev. Mr. Edkins, who believed Chinese obtained the 28 Mansions from Babylonia, identified it with the Babylonian word for Jupiter, "Dibbat Gutter."[34] The identification of a single name will not, however, solve this enigma. It is the nature of the cycle that is of importance. The cycle of Vrishaspati of 12 years as described by Parasara, was quoted by Varaha-Mihera:[35] "The name of the year is determined from the Nakshatra in which Vrishaspati rises and sets (heliacally), and they follow in order of lunar month. The years beginning with Cartic commences with Nakshatra Critica, and to each year there appertain two Nakshatras, except the 5th, 11th, and 12 years, to each of which appertain three Nakshatras." W. Brenand, the author of "Hindu Astronomy," also quoted the opinion of Davis to the effect that "The year Cartic is always placed the first of 12 Vrishaspati years, it may be inferred that there was a time when the Hindu solar year, as well as the Vrishaspati cycle of 12, began with the sun's arrival in or near the Nakshatra Cartic."

If we will compare these statements with those passages in "Wei-

Nan-Tze" and Se-Ma Tsien's "Historical Record," referring to the Chinese cycle of Jupiter, it is at once plain that the latter is but a reproduction of the cycle of Vrishaspati. Some of the Chinese names in the cycle like "Shitika," and "Ta-un-hin," still retain almost the exact transliteration of the corresponding Hindu Nakshatras "Critica" and "Dhanishtha" as shown in Table III.

W. Brenand also mentioned that "Now an astronomical work was known amongst the Hindus under the name of Brihaspati or Vrishaspati Siddhanta. . . That the name of the planet should have been associated with that of the sage Vrishaspati, would seem to imply a connection of this astronomical work with the Buddhist religion, and that in Siddhanta of Vrishaspati would be found rules which regulated the observance of Buddhist faith." It seems probable that the cycle of Jupiter was imported from India to China along with Buddhism, and this must have occurred in the period 300-200 B.C., when the cycle began to make its appearance in Chinese books.

TABLE III

A COMPARISON OF CHINESE CYCLE OF JUPITER WITH HINDU CYCLE OF VRISHASPATI

Hindu Year Name	Nakshatra	Chinese Year Name
Cartic	Critica, Rohini	Shitika
Agrahayan	Mrigaseras, Ardru	Shan-a
Baush	Punarvasu, Pushya	Chisu
Magh	Aslesha, Magha	Tafongloh
Phalgun	Purva Phalguni, Uttara-Phalguni, Hasta	Tunsang
Chaitr	Chitra, Swati	Hiphap
Vaisach	Visacha, Anuradha	Jundhan
Jaiscth	Jyeshtha, Mula	Tsongo
Ashar	Purna-Ashara, Uttara-Ashara	Immao
Sravan	Sravana, Dhaninshtha	Ta-un-hin
Bhadr	Satabahisha, Purna-Bhadrapada, Uttara-Bhadrapada	Kwantun
Aswin	Revati, Aswini, Bhorani	Chifangonk

C. THE ANTIQUITY OF CHINESE ASTRONOMY

Probably no exact date could be given to the origin of the system of 28 Mansions for the simple reason that the system was formed gradually, and did not arise *en bloc*. An estimate of age, however, can be made of the various asterisms or controlling stars which came to be known to the Chinese and later to be embodied into the system. According to Tung Tso-ping, the Chinese writings engraved on the divination bones and tortoise shells found in An-Yang excavation, three star names have already been deciphered: "Fire star," "Bird star," and "A New Great star," all in the age of Emperor Wu-ting (1329-1291 B.C.). Other star names or asterisms might be present but have not been deciphered so far. It may be inferred from the name of "Bird star" and "Fire star" that the scheme of dividing the heavens along equatorial circle into four figures of Blue Dragon (east), Red Bird (south), White Tiger (west),

and Black Tortoise (north) was already known at that time. This is about the only information we can derive from the archeological findings of An-Yang at present. There are, however, several indirect methods which may reveal the antiquity of the Chinese astronomy if not the system of 28 Mansions.

(a) *The position of sun and moon at solstices and equinoxes.* In Se-Ma Tsien's "Historical Record," it was definitely stated: "At the winter solstice the sun resides at Hiu."[36] This statement agrees well with Yao-Tien, according to which the equinoxes were in Taurus (Pleiades) and Scorpio, and solstices in Leo and Aquarius in the time of Yao. No doubt there was a tradition to this effect at the time when Shu-King (The Book of History) was compiled, for the author, knowing nothing of the precession of the equinoxes, could not have adjusted them to the time of which he was writing."[37] If we take the controlling star of Hiu as β Aquarii as given in Table I, the statement must be referred to a period 3800 years ago. The Hindus also had a tradition that formerly at winter solstice the sun had been in the middle of Aslesha, and at summer solstice in the beginning of Dhanishtha.[37] The fact that the 28 Mansions commenced with Kio in China probably indicates that the full moon was in the same Siu at Li-Chun or the commencement of spring at that time, which would date the origin of the system 1100 years still earlier, to the year 3000 B.C.

(b) *The location of celestial North Pole.* It is a well-known fact that due to the precession of equinoxes Polaris has not been always the north polar star to the inhabitants of the earth. During Se-Ma Tsien's time, β Ursae Minoris instead of α Ursae Minoris was the pole star, as can be verified by his statement in "Historical Record," and 2000 years earlier during the period when the Egyptians built the great pyramid of Cheops, it had been α Draconis.[39] But the early Chinese for some unknown reason did not use α Draconis, instead they employed two small stars in the neighborhood of α Draconis; "Tai-Yi" and "Tien-Yi." The names of these stars indicated that they were the north pole itself or "sovereign of the sky." The identification of these stars is not certain, they were mentioned first in "Sing-Kin" by Astronomer Si compiled about 400-300 B.C. The name Tien-Yi also occurred in Se-Ma Tsien's "Historical Record." Gustav Schlegel took them to be the star 30671, and κ Draconis; while de Saussure identified Tien-Yi to be ι Draconis.[40] The scope of this paper will not allow us to discuss the correctness of these identifications. Suffice it to say here that, according to the Chinese tradition of the former position of pole star, the antiquity of Chinese astronomy must go back to some time about 3000 years B.C.

(c) *The location of celestial equator.* With the passing of years the celestial equator moves with the celestial pole. J. B. Biot and de Saussure had hotly contended that the Chinese 28 Mansions were originally lying along the celestial equator, and that they were not lunar zodiacs,

as some writers called them, because they were not a belt of asterisms along the ecliptic. The truth of their contention can be envisaged if we take a glance at Schlegel's chart of 28 Mansions. The belt of these Mansions makes an angle of about 20° with the ecliptic.[41] This belt must have coincided approximately with the celestial equator of the period when the system of 28 Mansions had its origin. De Saussure believed it to be about 2500 B.C. The author of this paper has counted on a celestial globe the number of Mansions lying along the celestial equator at different periods, and found the following facts: At present there are eleven Mansions which are lying partly or wholly within a belt 10° on either side of the equator; at the beginning of Christian Era, 14 Mansions; between the period 2300-4300 B.C., 18 to 20 Mansions; at 6600 B.C., 15 Mansions; and at 8800 B.C., only 6 Mansions. The probability, therefore, is in favor of some date between 2300 and 4500 B.C.

(d) *The constellation Great Bear with nine principal stars.* The constellation Ursa Major was taken by the ancient Chinese as a standard clock to mark the time of the year; the tail of the "Great Bear" served as the pointer. There is a passage in Wei-Nan-Tze, which like the paragraph of Hoh-Kwan-Tze, quoted above, illustrates how the months and seasons can be told by noting the direction of the pointer of the clock. Wei-Nan-Tze mentioned the name of the pointer, a star called Chao-Yao, or γ Bootis. Thus it says, "When Chao-Yao points (at night fall) to Yin (N 60° E), it is the first month in spring. . . When Chao-Yao points to Mao (N 90° E) it is second month in spring, etc." Nowadays, the star Chao-Yao is not considered as a part of the "Northern Bushel." But there is a tradition mentioned in "Sing-Kin" and several other books of Taoist origin, that the constellation "Northern Bushel" originally consisted of nine principal star, two of them having been lost sight of in antiquity. These two stars apparently were alluded to λ and γ Bootis, which are located along the general direction of the handle of the "Northern Bushel." Now if the passage of Wei-Nan-Tze quoted above should represent the real fact, γ Bootis must then have been a circumpolar star, which appeared above the horizon all the year round. This would be the case only during the period 1500-4200 B.C.

(e) *The two stars Vega and Altair.* There are many legendary tales spun around this couple of stars which we need not go into. The interesting thing about them is that while the Hindus still kept them as Jogotaras in their Nakshatra, the Chinese displaced them with Nieou and Niu. More significant, however, is the fact that the order of the two asterisms has been reversed in the Chinese system as compared with the Hindu system. In the Hindu system Vega or Tse-Niu precedes Altair or Chien-Nieou, but in Chinese system Nieou goes before Niu. Now, judging from Chinese terms employed for these stars, Nieou should be the substitute of Chien-Nieou and Niu for Tse-Niu. But then

why this curious mixup? The explanation lies in the fact that, with the advance of time, the precession of equinoxes can bring about the change of right ascension of stars; and the magnitude of the change varies somewhat with different stars according to their position in relation to the ecliptic and the equinoctial colures. Controlling stars or Jogotaras of neighboring asterisms with small difference in right ascension but with great difference in declination may in time exchange places in the order of enumeration. A well-known case occurred in the 13th century orYuan dynasty in China, when the controlling star of Tsan, δ Orionis, overtook that of the preceding Siu, Tseu, λ Orionis.[42] This caused some confusion, and in Ming dynasty a great dispute arose among the court astronomers as to which Siu, Tsan or Tseu, should take the precedence. The question was finally settled by changing the controlling star of Tsan from δ Orionis to ξ Orionis, situated at the other end of the belt of Orion. Now exactly the same kind of inversion took place between the two bright stars Vega and Altair at about 3600 B.C. Se-Ma Tsien was unaware of this inversion of order, so he said in the "Historical Record," "Chien Nieou (now Nieou) is for sacrifice, farther north the Aquila; bright star in the center, the general; one each on its right and left, lieutenants. Then followed by Wu-Niu (now Niu) farther north, Tse-Niu who is a heavenly princess." We may, therefore, conclude from the facts stated above that when original arrangement was made more than 5000 years ago, Vega was still east of Altair in right ascension, hence the latter preceded the former. After 3000 B.C. the order was reversed. Ignorant of this fact, Se-Ma Tsien in enumerating the different Sius still mentioned them in the old order as was handed down traditionally. The Hindus, borrowing the system from China before Altair and Vega were substituted by Niu and Nieou, persisted in using them, but reversed their order. The Chinese, using the substitutes, Nieou and Niu which differ not much in declination, keep them in the old order.

(f) *Rain God Pi and Wind God Ki.* In the Chinese "Book of Poetry" there stand the following lines:[43]

> There are the swine with their legs white,
> All wading through streams,
> The moon also in Hyades,
> Which will bring still greater rain.

So far no satisfactory explanation has been offered to elucidate why the moon in Hyades should forebode rain in China. Although many Chinese commentators of the classics had tried their wit in giving interpretations to this passage, none could stand any examination. In "Uranographie Chinoise," Gustav Schlegel offered an explanation which was on the right track to solve this puzzle. But owing to his bias toward high antiquity of the Chinese astronomy, he went amiss. He said "Ces pluies avaient lieu quand la lune entrait dans l'asterisme Py; mais

dans l'époque historique cette coincidence des pluie avec la conjonction de la lune et de cet asterisme n'avait plus lieu; car, à l'époque du Chouking, la nouvelle lune entrait dans les Hyades au printemps et pas en automne, époque des pluies torrentielles."[44] Kingsmill was also greatly puzzled by it. He said, "The connection of the Hyades with rain was early believed by the Chinese, the "Book of Poetry" tells us that when the moon passes the Pi there will be heavy rain, a statement, however, difficult to reconcile with the observations."[45] The Rev. J. Edkins poses the question: "Why does the constellation Pi, the Hyades, the rainy stars, indicate rain?"[46] That Hyades should be looked upon as rain God by both Chinese and Western people, like Babylonians and Greeks, is certainly remarkable. The reason why the Chinese should regard moon in the Mansion Pi as a forerunner for rain, however, can be explained, if we denote the moon as full moon, which is what it should mean. About the period 4000 B.C. the full moon entered the asterism Pi by the middle of August when rainy weather prevails in northwest China. The asterism Ki was usually taken as the Wind God, this was alluded to in the Chinese classic Shu-King (Book of History). This fact can be explained on the same ground, *i.e.*, at the period 4000 B.C., the full moon entered the asterism Ki at vernal equinox, when the wind velocity usually attains its maximum in northwest China.

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Some Ancient Solar Observations

By EDWARD G. SCHAUROTH

Part I

Recently, in the course of some rambles undertaken for my own amusement through the pages of the old geographer Strabo, my attention was held by certain references to observations of sun positions. Approaching these records with the spirit of the navigator rather than that of the critic, I found that certain conclusions could be drawn from the data, which, seemingly, have hitherto escaped the notice of more

learned commentators. It is in the hope that the speculations which have provided me with many hours of profitable entertainment may prove interesting to others that I venture to set forth some of them.

Some two hundred and fifty years before Strabo's time, the great astronomer and mathematician Eratosthenes was engaged in his researches at Alexandria. As head of the school connected with the Museum, he enjoyed the patronage of the reigning Ptolemy and could devote his life to the pursuit of knowledge. One of his chief interests was geography, and to the development of this science his astronomical discoveries contributed much. His authentic works are lost, but in ancient times their repute was such that a man in Strabo's day could not afford to make unorthodox assertions in the field of geography without alluding to the name of his great predecessor. Hence, the multitudinous references to Eratosthenes in Strabo.

Eratosthenes' chief claim to fame in later times was the great experiment he carried out to determine the length of the earth's circumference. Others before his time, including Aristotle, had hazarded a guess, but Eratosthenes was the first to devise a simple, accurate method. It had long been known that the earth was a sphere, and Eratosthenes made the bold assumption that the sun is far enough away so that its rays will fall parallel upon two distant points. The zenith arc measured by the observer is, therefore, equal to the arc on the earth's surface between the point of observation and the geographic position of the sun. The following diagram will serve to illustrate the principle:

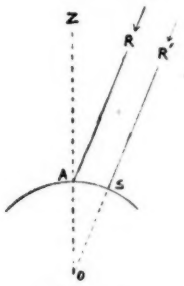

FIGURE 1

S represents the sun's geographic position or the point where the sun is in the zenith; A, a point on the same meridian to the north; Z, the zenith of the observer at A; R and R′ represent the sun's rays falling parallel. Since the arc AS is the measure of the central angle AOS, and since the angle RAZ is equal to the angle AOS, the arc AS is also the measure of the angle RAZ. Therefore the angle RAZ in degrees bears the same ratio to 360° as the length of the arc AS bears to the length of the earth's circumference.

The principle here involved is the same as that employed by the navigator today, who determines his latitude by taking the sun's altitude at the instant of meridian transit. Knowing the sun's declination, he calculates the altitude of the celestial equator at the meridian. The complement of this angle is the angle between the zenith and the equator on the meridian and corresponds to the central angle which measures his latitude or angular distance from the equator.

Upon the theory thus outlined, Eratosthenes proceeded to his great experiment. He possessed information to the effect that on the day of the summer solstice a gnomon in the city of Syene (modern Aswân) in Upper Egypt, almost due south of Alexandria, cast no shadow at noonday. Preparations were made at Alexandria to measure the zenith angle of the sun at solstitial noon. By the skilful measurement of sun shadows this angle was determined as 1/50 of the total angle about a point.[1] All that remained was to establish the distance between Syene and Alexandria. If we are to believe one account, this was measured expressly for him;[2] but it seems far more likely that the only information available was that which came from merchants and travellers who had made the journey. The distance was put at 5000 stades. 5000 stades measured on the meridian as 1/50 of a circumference would yield a circumference of 250,000 stades, but we know that Eratosthenes' estimate was 252,000 stades.[3] We shall consider the length of the stade later.

The difference between 250,000 and 252,000 stades is a discrepancy that may be explained variously. It may be that the number 5000 represents a figure rounded off for the sake of brevity, or that Eratosthenes knew that Syene was not exactly on the northern tropic. At any rate, the figure 252,000 is substantiated, through a fragment we have of Hero, as being Eratosthenes' estimate.

Another great achievement of Eratosthenes was the determination of twice the angle between the planes of the ecliptic and equator. This is represented by the angular distance of the sun's path between the tropics, or more simply, by double the latitude of either tropic. Eratosthenes' estimate gave this arc as 11/83 of a circumference,[4] or 47° 42′.6 in our notation. Whether this value was derived from direct observation made at the tropic or was the result of calculation, we are not told. Little is known of Eratosthenes' personal history and the bits of information we have about him are recorded by much later writers. We shall return to this question presently to seek an answer.

Before attempting any evaluation of Eratosthenes' data, it may be well to adopt an attitude of approach. In some of the handbooks dealing with the subject I note that the commentators first insist on applying all the refinements of modern astronomical measurements to show how far wrong Eratosthenes was. Then they proceed to feel extremely sorry for him because his measurements were not exact; and finally they are filled with amazement at the fact that his calculations are as

good as they are. I hope that I state the case not unfairly. What I am interested in determining is how closely Eratosthenes could measure an angle. His reputation is well enough established to remain unaffected by any words of praise or criticism from me.

In the first place, we are frank to admit that Eratosthenes knew nothing of the effects of refraction upon celestial observations. He knew nothing of the oblateness of the earth or, consequently, of the slight variation in distance, as one proceeds from the equator to the pole, between parallels measured by equal central angles. He may, or may not, have known that Syene and Alexandria are not on the same meridian. To bring up such criticisms of his work is puerile. After all, the navigator gets along very well by assuming the earth to be a perfect sphere and making his calculations according to the laws of spherical geometry and trigonometry. Any errors introduced by this assumption are for him negligible. For him, a nautical mile is a minute of arc on any great circle, and the earth's circumference, whether meridionally or equatorially, is 21,600 such miles. For the higher sun altitudes determined by Eratosthenes, the correction for refraction is a small part of a minute of arc; for the lesser ones, about a minute. Even less is the correction for parallax, and the effect of the two is such that they tend to cancel each other.

Astronomers have observed that, owing to the influence of the moon and the nearer planets, the angle of the ecliptic to the equator changes over the centuries. This means that the parallels of the tropics undergo the same change. In our time the tropics are 23° 26′ 44″.84 from the equator. Around 250 B.C. when Eratosthenes made his observations, the tropics were at an angular distance of 23° 44′ from the equator,[5] these being the northernmost and southernmost limits of the sun's path. Since the calculation is subject to a possible error involving decimals of a minute, the result is given to the nearest minute of arc. We shall deal similarly with the other values under consideration. Twice 23° 44′ is 47° 28′, the angular distance between the tropics. It is easy to see that Eratosthenes was wrong with his value of 47° 43′, but, before we dismiss his data, let us see what sources of error they may reveal.

To measure the width of the tropic zone, an observation of the sun at the winter solstice was necessary. The question arises as to where this observation was made. It is indeed strange that no record giving the data of so critical an observation should be preserved. In the absence of definite knowledge, some scholars have been quick to assume that Eratosthenes made his measurements at Syene. It is true that Syene was thought to lie on the tropic and Strabo still records that as its position, assuming its latitude to be 24° N.[6] This locality might appeal to an observer, for the value here obtained for the sun's zenith angle at the winter solstice would be identical with the distance between the tropics and no further observation would be required. But Eratos-

thenes could not have made his observation at Syene under the assumption that he was at the tropic, for, according to his own computation, he placed the tropic in 23° 51′ N and Syene is actually 24° 5′ N. There could be no possible advantage in making the observation at Syene rather than at Alexandria, for in either case another observation at the summer solstice would be necessary. Furthermore, how could the astronomer, unless prepared to sacrifice years by experimenting on successive solstices, be sure he was exactly on the tropic? There was not the slightest need for so laborious an undertaking. No, the observation must have been made at Alexandria where all facilities to insure accurate work were available. The result obtained at the winter solstice could then be combined with the result of the observation made at the summer solstice. The figure will illustrate the principle.

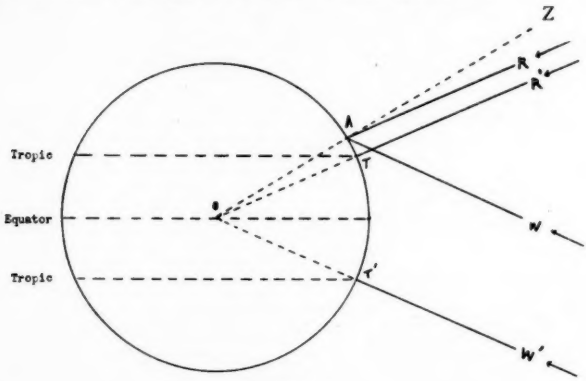

FIGURE 2

Let R and R′ represent the sun's rays falling parallel at the summer solstice, the sun being vertical at T, the northern tropic. Let W and W′ represent the sun's rays falling parallel at the winter solstice, the sun being vertical at T′, the southern tropic. At Alexandria (A) the angle RAZ represents the sun's zenith distance at the summer solstice; the angle WAZ, the sun's zenith distance at the winter solstice. The arc TT′ is the width of the tropic belt and the measure of the central angle TOT′, the value of which is required.

$$\angle TOT' = \angle AOT' - \angle AOT$$

But

$$\angle AOT' = \angle WAZ \text{ and } \angle AOT = \angle RAZ$$

Therefore

$$\angle TOT' = \angle WAZ - \angle RAZ$$

The zenith angle WAZ, established by direct observation, is, of course, equal to the sum of RAZ and WAR (= TOT′). The result of Eratosthenes' work gave their values as 7° 12′ and 47° 43′, respectively.

Their sum, 54° 55′, must have been the value of the angle which he obtained. Let us now examine its significance in the light of what we can assert were the physical conditions which determined it.

Refraction would cause the sun, at the altitude observed, to appear about a minute higher than it actually was. The true angle representing the observer's distance north of the southern tropic would thus be 54° 56′. To get his latitude, we subtract the latitude of the tropic. Knowing this to have been 23° 44′ in Eratosthenes' time, we obtain 31° 12′. The result is startling, for not only does this value fit the general latitude of Alexandria, but it is exactly that of the Island of Pharos! There, at the entrance of the harbor, stood the great lighthouse, then but recently built, that was counted as one of the seven wonders of the world. It may well have served the astronomer as a laboratory for manipulating his precious sun shadows. But before we can accept conclusively the theory that this was the seat of Eratosthenes' labors, we must seek an explanation of the errors in his calculation.

To maintain that Eratosthenes could measure angles to a degree of accuracy involving an error of less than a minute of arc, may seem to some like the romancing of a poetic imagination. "Why," we are asked, "did Eratosthenes not know the width of the torrid zone, if he could measure thus accurately?" The answer is quite simple and should appeal to the imagination of the navigator, for the correction involved is that of the semi-diameter of the sun.

In his famous attempt to measure the circumference of the earth, Eratosthenes had to determine the zenith arc of the sun at the summer solstice. This, we saw, amounted to 7° 12′. But 7° 12′ plus the sun's maximum declination, 23° 44′, gives us 30° 56′ for the latitude of Alexandria, exactly 16′ too little. At once we recognize in this figure the value of the sun's semi-diameter. What by reputation was the greatest of Eratosthenes' measurements was at fault by just that amount. Of this there can be no doubt. Had he made the appropriate correction or obtained the true angle by some other means, it would necessarily have been found to be 12° 28′, and this figure deducted from the correctly ascertained angle between Alexandria and the southern tropic would have yielded the correct width of the torrid zone.

It is beyond question that Eratosthenes needed to make an observation at the winter solstice in order to determine the width of the torrid zone. The results of this observation we have reconstructed and have found them faultless. The more surprising is it that what is truly the greatest of Eratosthenes' achievements in the art of measuring celestial angles has been overlooked by scholars.

By this time the reader's curiosity may have been aroused as to precisely the method followed by Eratosthenes for determining his solar angles. Unfortunately, we have no contemporary account, and the method described by Cleomedes, a late writer, may well be colored by

his knowledge of the methods followed by later investigators, for this classic experiment was often repeated. We are told that bronze cups were used, provided with a stilus at the center to cast a shadow, and carefully graduated so as to make possible the reading of the shadow's angle. Much has been written by modern critics as to the amount of error that might be introduced into the measurements, accordingly as to whether umbra or penumbra was read. Again I am moved to view with suspicion the extreme refinements of the arguments. I do not say that I know exactly how Eratosthenes made his measurements, but I believe it is possible to point out methods whereby he might well have achieved the degree of accuracy I have ascribed to his credit.

I find it difficult to believe that, where accuracy of measurement was a prime requisite, Eratosthenes should have relied upon any device as small as the traditional bowls. The longer the shadow to be measured, the more reliable would be the result. A darkened chamber would seem to provide far better means to this end. From an aperture in the roof a plumb line could be dropped to ascertain the point directly below. The sunbeam thrown by the direct rays of the sun on the floor could be traced in its course over the noonday period. Of course, the floor must be level, but, even if it were not exactly so, the error would be slight for high sun altitudes. An eighth of an inch is an appreciable and readily measurable distance. To mark a displacement from the vertical of one minute of arc by this amount, a roof about thirty-six feet high would be required. For lesser altitudes the displacement effected by a change of one minute of arc becomes rapidly greater. We know that by the time of Hipparchus, about a century after Eratosthenes, an elaborate system of trigonometry had been developed. It is not too much to assume that, from careful measurement of the sunbeam's displacement from the vertical, the zenith angle of the sun could be calculated to a nicety.[7]

This is mere speculation, but I believe it has some bearing upon an interpretation of Eratosthenes' findings. Of the two critical observations, we have seen that one was exactly right and that the other was in error by an amount equal exactly to the sun's semi-diameter. We may infer that two methods were employed, for otherwise it would be natural to look for the same error in both results. Since at least half a year must have elapsed between the observations, the inference is the more plausible. Now it is significant that just the observation which received so much publicity, because from it the earth's size was calculated, was the erroneous one; and that the other, of which no mention is made but which is basic to the measurement of the torrid zone, should be correct. Now a method such as that which has been described, employing either a long ray of light or a long shadow, need not take into account the sun's semi-diameter, because the center of the sunspot can be marked with sufficient accuracy by eye alone not to vitiate the result.

Such would not be the case, however, if a shorter line of measurement were taken. The astrolabe had by this time been perfected into something of a precision instrument, and I suspect that the error we have noted may have been due to its use. In pointing the instrument, it is possible that the zenith angle to the sun's upper limb was measured.

Setting conjecture aside, we are content to have shown the degree of precision with which Eratosthenes could and did measure celestial angles as well as the source of error which entered into his calculations. His measurements bear the right proportion to each other and are in agreement with the geographical and astronomical facts, if a single allowance for the sun's semi-diameter is made for one of them. If this is not construed as a coincidence, we must accept the association of the Island of Pharos with some of Eratosthenes' most important work.

NOTES TO PART I

[1] Cleomedes, I, 10.
[2] Martianus Capella, VI, 598.
[3] Strabo, II, 7 (p. 113). (The page number being that in Casaubon's *edito princeps.*)
[4] Ptolemy: *Syntaxis*, I, 12.
[5] Encylopaedia Britannica, 11th ed. (1910-11), article, *Ecliptic.*
[6] Strabo, II, 7 (p. 114).
[7] I have myself used this method for the experimental determination of latitude with surprisingly accurate results, even though the diaphragm with its aperture was raised but a few feet above the floor.

PART II

From early times reports of Britain and the Northern Ocean had excited the imagination of the Greeks, yet so fragmentary was the information that reached them, that long even after Strabo's time the shape of Britain was wholly misconceived. Strabo thought of it as an island lying off the coasts of Gaul and Germany, its extent in that direction greater than to the northward. Ireland, he imagined, lay beyond Britain to the north.

About the time of Alexander the Great, or later, for we do not know his date, an explorer from Massalia (Marseilles) in Gaul, named Pytheas, sailed to Britain and, claiming to have reached its northernmost point, reported that he had sailed for six days into the sea to the north. He described his experiences in a book that he wrote after his return. Unfortunately, it is lost, but some of his statements are preserved for us through the writings of his enemies who believed the account a nefarious fabrication and its author an impostor. Strabo is one of the skeptics of a later day and, while he repeatedly calls Pytheas a liar, he allows us here and there to catch a fleeting glimpse of the old voyager's narrative. From what we can discern, it would appear that Pytheas was a faithful chronicler of what he saw, and that the wonders of which he told were merely such phenomena as were unfamiliar to the armchair travellers of his time. In recent years Vilhjalmur Stefans-

son has offered the world a stalwart vindication of the character of this great explorer.[1]

In the first part of his work Strabo discusses the size and general outline of the "inhabited world." The scarcity of available and reliable information necessitates for him a lengthy course of argument to determine what lands are on similar parallels of latitude. Constant reference is made to the work of Eratosthenes and Hipparchus in this field. In a certain passage of Chapter 18, (Book II), allusion is made to some sun altitudes observed by Pytheas in the far north. It is to them that I would direct attention, to discover, if possible, with what degree of accuracy they were made and what they may prove for the claims of the ancient navigator.

My translation of the passage is as follows:

> Hipparchus says that in the latitude of the Borysthenes and Gaul there is a shimmering of the sun's light all through the summer nights, shifting from the west to the east, and that at the winter solstice the greatest elevation of the sun is nine cubits; furthermore, that in the regions 6300 stades distant from Massalia (which he supposes to be in Gaul, but I think are 2500 stades north of Gaul), this is much more noticeable. In the winter days the sun has an altitude of six cubits there; of four cubits in the regions 9100 stades distant from Massalia; and of less than three in the parts beyond, which, according to our account, would lie much farther north than Ireland. Hipparchus, relying upon Pytheas, puts this region farther north[2] than Britain and says that there the longest day is of 19 equinoctial hours' duration, and of 18 hours', where the sun's altitude is four cubits. The latter regions, he says, are 9100 stades distant from Massalia . . .

A few comments may be in order before we proceed to analyze the passage. The river called *Borysthenes* is the Dnieper. The word translated *cubit* is *pêchys,* regularly a unit of length. That it is used here with any such notion is quite evidently impossible. Only a unit of circular measure can be indicated. By *equinoctial hours* are meant hours of such duration that 24 will constitute a full solar day. Since it was practice in ancient times to divide the period of daylight into twelve hours of equal length, the length of an hour would vary with the season of the year. At the equinoxes, when the period of daylight is of the same length as the night, the hour was virtually the same in length as our hour representing 1/24 of a mean day. Any attempt to define it more precisely would be futile, for there were no timepieces known comparable to ours in accuracy.

The passage is part of a longer argument in which Strabo tries to prove Hipparchus wrong in certain essential assumptions. Here he admits the truth of the sun altitudes cited at specified distances north of Marseilles. What he does not concede is that the latitude at which the sun has an altitude of four cubits is in Britain. Pytheas had said so, and Hipparchus apparently accepted his simple statement. Did Hipparchus and Strabo merely calculate the latitudes at which the day was of 18 hours' and, again, of 19 hours' duration? That Strabo, at least,

was checking up on his predecessors seems clear. Hipparchus may have done so too, but I believe that mention of the day of 18 hours and of the day of 19 hours goes back to the record of Pytheas.

In his account Pytheas would have said that he observed the sun's altitude at four cubits in a certain part of Britain and, like any careful explorer, would have added that here the day was of 18 hours' duration. That would constitute convincing proof in the mind of any doubter that Pytheas had been where he claimed to have been if the skeptic took the trouble to check up his data. Little as is known about the man, there can be no question of the fact that his voyage was undertaken purely for the sake of discovery and that his purpose was what the modern world would call "scientific." The careless phraseology of Strabo may obscure the issue and at first sight lead us to think that Hipparchus alone is responsible for the statement that at a certain latitude the sun's altitude is four cubits; at another, less than three; and that these places have days of maximum length respectively of 18 and 19 hours. After all, Strabo is challenging Hipparchus' views, and, having long since put Pytheas in the category of a liar,[3] discredits Hipparchus by representing him as Pytheas' dupe.

Since we are as uncertain about the value of the stade as we are about that of the cubit, can we reach any conclusions regarding the latitudes indicated? If so, we shall at the same time have approximate values for stade and cubit. Let us begin with a study of the natural phenomena recorded.

Bearing in mind that the sun at this period had a maximum declination of 23° 44′, we can calculate the lowest latitude at which twilight would continue through the night for the days near the summer solstice.[4] This brings us to a latitude over 48° 16′ N. Similarly, the day of 18 hours brings us to 57° 2′ N;[5] the day of 19 hours, to 60° 0′N. Even simpler is the calculation of the sun's altitude for these latitudes at the time of the winter solstice.[6]

It will be noted that in making his observations Pytheas must have spent a goodly time in the north, a fact that adds great weight to the argument for his veracity. We shall not expect that with the instruments at his disposal he achieved as high a degree of accuracy as did Eratosthenes who was working under favorable conditions; nor, that every critical observation was made exactly at the solstice. Let us, however, for the sake of the argument, assume that when he said four cubits, he meant exactly four as he measured them; for the cubit is a large unit for measuring sun altitudes. The following table summarizes the results outlined. Apparent sun altitudes have been calculated to the nearest whole minute.[7]

Phenomena of Latitudes	Latitudes	Sun's apparent altitude at winter solstice	Apparent altitude in cubits	Value of cubit
19 hour day	60° 0′ N	6° 22′	under 3	over 2° 7′
18 hour day	57° 2′ N	9° 20′	4	2° 15′
continuous twilight	about 48° 30′ N and over	17° 49′ or less	under 9	about 2°

We shall next attempt to ascertain the sun's altitude at the regions designated as 6300 stades from Marseilles. Since the observations represent apparent altitudes, we must proceed from them. Now the required altitude of the sun for the latitude designated as 6300 stades from Marseilles bears the same relation to 6 as 9° 20′ bears to 4. This comes to 14° 0′. The true altitude of the sun would then be 13° 56′, and the latitude of the place, 52° 20′. Taking 14° 0′ as the equivalent of 6 cubits gives a value of 2° 20′ for the cubit.

The discrepancies in the values thus obtained for the value of the cubit need not surprise us, considering the fact that we have based our calculations upon a latitude corresponding to a day of exactly 18 hours. It is likely that Pytheas could measure angles far more accurately than he could record the passage of time. At any rate, such is our assumption. We have obtained an approximate value for the cubit and may proceed to work from it. Before doing so, however, some reflections of a general nature may prove helpful.

The prime requisite of any unit of circular measure is that it be evenly contained in a whole circumference or its major divisions. In measuring the altitudes of celestial bodies we constantly refer them to the quadrant. The cubit, in order to fulfill its proper function when used in astronomy, must have represented a convenient division of the quadrant. That it should have any other value seems to me unthinkable. Now in the close vicinity of 2° we find only one magnitude into which a quadrant is conveniently divisible. That is 2° 15′, which is contained 40 times in a quadrant.

The magnitude 2° is tempting. Eratosthenes may have accepted the figure 252,000 to express his estimate of the earth's circumference because that number is evenly divisible by 2, 3, 4, 5, 6, 7, 8, 9—to mention only the first of its series of many factors. A quadrant of a great circle could be expressed by 63000, and a degree by 700. It is to be remarked that the figures cited by Hipparchus and Strabo for the distances of certain localities north of Marseilles are 6300 and 9100, both of them multiples of 700. This has evidently led others to the assumption that their difference, 2800, represents 4° of latitude, 700 stades being reckoned to a degree; and if a difference in latitude of 4° causes a change in the sun's altitude of 2 cubits, then the cubit is equivalent to 2°.

The following table will show the latitudes we arrive at if we start with the assumption that the cubit is (1) 2° or (2) that it is 2° 15′.

Sun's apparent altitude in cubits	(1) True altitude for value 2°	(1) Corresponding latitude	(2) True altitude for value 2° 15′	(2) Corresponding latitude
8½ to 9	16° 57′ to 17° 57′	48° 19′ to 49° 19′	19° 4′ to 20° 13′	46° 3′ to 47° 12′
6	11° 56′	54° 20′	13° 26′	52° 50′
4	7° 53′	58° 23′	8° 54′	57° 22′
2½ to 3	4° 50′ to 5° 52′	60° 24′ to 61° 26′	5° 28′ to 6° 37′	59° 39′ to 60° 48′

From a study of the figures above it is apparent that the value 2° 15′ for the cubit brings us closer to our calculated latitude of 57° 2′ than does the value 2°. Likewise, an altitude of 2½ to 3 cubits, reckoned at 2° 15′ to the cubit, covers the parallels of which 60°, calculated for the 19 hour day, is almost mid-latitude. Counting 2° to the cubit takes us too far north both in the case of the four cubit observation and that of the six cubit observation. In fact, at 58° 23′ the day is one of 18 hours and 25 minutes, and at 60° 55′ (the mean of 60° 24′ and 61° 26′) it is of 19 hours and 23 minutes. In all probability days of such length would have been reported as of 18½ and of 19½ hours' duration, respectively. It is to be admitted that our latitude of 57° 22′ is also north of the parallel we calculated for an 18 hour day; but at 57° 22′ the day is only between five and six minutes longer than this, an amount that may well have been disregarded, if, indeed, it was known. On the other hand, assuming 2° as the value of the cubit in this observation, which must have been made from the vicinity of 57° N to satisfy the length of day, we find that the altitude is such that it would have been reported as of five rather than of four cubits.

We have been working our way toward the vaguest of our data. Where twilight lasts from sunset to sunrise we are to expect an altitude of the sun at the winter solstice of less than nine cubits. Only here does the value of 2° for the cubit fit better than the value 2° 15′. From 46° to a little over 47° N, there can be no continuous twilight. That can only occur above 48° 16′. We note, however, that the authority for the assertion is Hipparchus. If my assumption is correct that the altitudes given in conjunction with the latitudes designated by the length of the twilight hours near the summer solstice are to be referred to Pytheas, this one is certainly not supported by him. Hipparchus was discussing correspondence in latitude between localities in the west and in the far east. That his information was faulty is amply borne out by what we know of his ideas expressed elsewhere. So far, we have discovered nothing to invalidate the data we have assumed originated with Pytheas. That his data are not corroborated by reports from other sources, but are independently borne out by astronomical phenomena which we can verify, may be taken as added proof that our theory is justified.

It would be absured to maintain, after our calculation of altitudes and latitudes to a minute of arc, that Pytheas did the same and that he was actually at 57° 22′ N when he observed the sun at four cubits. The amazing thing is that our calculations thus made should correspond so closely with the reported data. Since they do, we must infer that when Pytheas said four cubits he meant exactly four by his observation, and that when he said eighteen hours he meant that lapse of time as closely as he was able to measure it. When all due allowance has been made for inaccuracy in the measurements, we can at least place him with confidence between the limits of 57° 2′ and 57° 22′ N on the coast of Britain, when he observed the sun at four cubits; and between 60° 0′ and 60° 13′ N, when the lower altitude was observed.

That work of so accurate a nature as this must have been done on land, goes almost without saying. It is interesting to note that our limits for the northern position both cross the Shetland Islands and place Pytheas not far from Lerwick. The southern position, if Pytheas landed on the east coast of Scotland, must have been within a few miles of the present site of Aberdeen. A chart will show that the two places are almost on the same meridian, a suggestive fact when we recall the explorer's bold resolve to push out into the sea to the north. Not far north of Aberdeen the coast turns west, leaving the open sea between there and the Shetlands. Fanciful conjecture may thus give its preference to the east coast.

Part III

We are now ready to consider the stade in relation to the record of Pytheas' sun observations. By reconstructing the method followed by Hipparchus in using such data, we may discover what value he attached to the stade and the latitude he ascribed to Marseilles.

Hipparchus, of course, knew nothing of the effects of atmospheric refraction upon the measurement of celestial angles. We must therefore deal with the apparent altitudes of the observations we have interpreted. Ptolemy tells us that Hipparchus used the same value for the angle between the tropics as that determined by Eratosthenes.[8] Half of this, or 23° 51′, would represent the assumption for the sun's maximum declination. The altitude of four cubits observed by Pytheas would then indicate a latitude of 57° 9′ N, and the altitude of six cubits, 52° 39′ N. These parallels are stated as being respectively 9100 and 6300 stades from Marseilles. Now 57° 9′ less the latitude of Marseilles must bear the same relation to 9100 as 52° 39′ less the latitude of Marseilles bears to 6300. Solution of the proportion yields 42° 32′ for the required latitude. This determination differs considerably from the true latitude, 43° 18′ N; but even Ptolemy, some two hundred years after Hipparchus, gave the latitude of Marseilles as 43° 5′.[9] Amid the many errors that may be noted in the latitudes ascribed to familiar

places in the Mediterranean world, the determination of his latitudes in the far north by Pytheas stands out as a brilliant achievement in the annals of navigation.

Whether Hipparchus by our standards knew, or did not know, the magnitude of the earth's circumference and, therefore, the length of a degree of latitude, he must have had a value in his own notation for this distance. If we take the latitude of Marseilles as 43° 32′, it is 14° 37′ from there to 57° 9′; and 10° 7′ from there to 52° 39′, distances respectively of 877 and 607 nautical miles. If the first is equivalent to 9100 stades and the second, to 6300 stades, the value of the stade is 585 English feet. This result is particularly striking to any one who has tried to reconcile distances reported by ancient geographers with any of the various kinds of stades which we know were employed. Our result differs by only three feet from the length of the Old Attic Stade, one of the commonest of the large family of stades in use.

If we adopt 582 feet, the length of the Old Attic Stade, as the value of Hipparchus' stade and start counting 9100 stades south from 57° 9′ N, we arrive at 42° 38′ for the latitude of Marseilles.The error in Hipparchus' calculations from the accepted latitude of Marseilles is thus seen to be no great one, involving, possibly, six minutes of arc in a distance of fourteen and a half degrees. The error lay in his apparent acceptance of the latitude of Marseilles as it was given by others.

* * * * *

The arguments that have been presented in the foregoing discussion relative to the measurement of celestial angles in ancient times are admittedly circumstantial in nature. The theories suggested would hardly deserve attention if proposed merely as theories; but their correspondence with verifiable physical phenomena and accurate modern measurements is such that this correspondence in case after case cannot reasonably be accepted as a series of fortuitous coincidences. In spite of the scanty evidence that has reached us, some of it distorted by the opinions of later writers, we see that as early as the time of Eratosthenes the ancients could and did measure celestial angles to an accuracy of a minute of arc under favorable conditions; and that under decidedly unfavorable conditions observations made by men like Pytheas may convince the modern world of the truth of the navigator's claims to have been where he said he was, whereas the same evidence was branded false by his contemporaries. By this very evidence the latitudes of two of Pytheas' observations have been fixed within comparatively narrow limits and the seat of Eratosthenes' astronomical work has been placed upon the Island of Pharos.

NOTES TO PARTS II AND III

[1] Vilhjalmur Stefansson: *Ultima Thule*, Macmillan, N. Y., 1940.

[2] It is unfortunate that the passage in question should be marred by a textual difficulty. The preferred reading is "farther south." To reopen the controversy

is beyond the scope of this article. Suffice it to say, that Hipparchus' conceptions of geographic positions are not the immediate subject of inquiry and have no bearing upon the significance of Pytheas' observations. While some scholars regard the authenticity of the reading "farther south" as proved, since a subsequent remark seems to bear it out, I cannot bring myself to accept it for the reason that in that case both Hipparchus and Pytheas are represented as putting the southern coast of Britain somewhere above 60° N. This supposition seems to me so preposterous, that I have omitted part of the last sentence from my translation.

[3] Strabo: pp. 64; 102; 115; 158; 190; 201; 295.

[4] The *American Nautical Almanac* reckons twilight as continuous when the sun's true zenith distance is 108° or less from a given locality (p. 307, ed. 1946). The formula is, therefore, CO-LATITUDE + CO-DECLINATION must not exceed 108°.

[5] To calculate the latitude at which the period of daylight is 18 hours long, the elements of the spherical triangle PZM have the following values: P = 135° (½ of 18 hours in arc); PM = 66° 16′ (co-declination); ZM = 90° 50′. The last figure is obtained by adding 34′ for refraction and 16′ for the sun's semi-diameter to 90°, the apparent zenith distance of the sun's upper limb at sunset (see *American Nautical Almanac for 1946*, p. 307). The other calculations follow readily.

[6] Since the altitude of the equator on the observer's meridian is equal to his co-latitude, and the sun's declination in this instance is south, we have: ALTITUDE = 90° —LATITUDE —DECLINATION.

[7] Corrections for mean refraction and parallax are taken from the tables of the *American Nautical Almanac* and given to the nearest whole minute. For low altitudes observed under the variable weather conditions of the far north, it is evident that the index of refraction may vary from that given for a mean temperature and a mean barometric pressure. However, we can hardly do better than accept the mean values for our purpose.

[8] Ptolemy: *Syntaxis*, I, 12.

[9] Ptolemy: *Geography*, II, 9.

University of Buffalo.

"Versed Ascension": A Substitute Term for "Sidereal Hour Angle"

By FREDERICK C. LEONARD

Under the title, "A New Name for the Sidereal Hour Angle," the following unsigned note appeared in *Navigation* (the "Journal of the Institute of Navigation"), **1**, No. 4, 92-3, December, 1946:

"At the June meeting of the Institute, a lively discussion took place regarding the naming of the coördinate commonly called in the United States the sidereal hour angle. No one defended this name, and it was attacked substantially on the basis expressed by Frederick C. Leonard, who called it 'the unhappily so-called "sidereal hour angle"' in an article in Popular Astronomy,[1] where he goes on to say:

"'The term "sidereal hour angle," which apparently was first employed in *The American Air Almanac* for 1941, is inappropriate, (1) because the coördinate in question is not an hour angle and (2) because the concept is applicable not only to stars but to all kinds of celestial objects; *cf.* the discussion of the matter in Paul E. Wylie's *The Essentials of Modern Navigation*, 5th Rev. Ed., 262-3, 1943. Mr. Wylie has

recently proposed in conversation a really pertinent term, *right descension* (symbol, *b*), to supersede the expression "sidereal hour angle." . . .'

"Gordon Atwater emphasized that the term sidereal hour angle is inappropriate in that, contrary to the legitimate expectation of the novice, the coördinate described has no direct connection with the measurement of time lapse.

"L. J. Comrie, former Director of the British Almanac Office, calls the sidereal hour angle 'that supreme example of meaningless nomenclature.'

"There seems to be no lack of logically derived substitute names. In addition to Wylie's suggestion, the term versed ascension is proposed, and is in actual use by the French. Perhaps some change in American usage may be made before sidereal hour angle becomes imbedded in the literature, and we are confronted with as unfortunate a situation as the one with which we shall always have to contend in the use of the same terms for minutes and seconds of arc and of time."

I want to add my hearty endorsement of the term *versed ascension*—for which the symbol *v* (not likely to be confused with that for "velocity" or "true anomaly"), *v. a.*, or *V. A.* might be used—as a replacement for what Comrie has so justly called "that supreme example of meaningless nomenclature"—"sidereal hour angle."

REFERENCE

[1] Leonard, Frederick C., "The Amplement and the Coamplement of an Angle," POPULAR ASTRONOMY, **53**, No. 3, 121-2, March, 1945.

DEPARTMENT OF ASTRONOMY, UNIVERSITY OF CALIFORNIA, LOS ANGELES,
1946 December 16

The Planets in March, 1947

By LELAND E. CUNNINGHAM

NOTE: The time employed is Central Standard Time unless otherwise indicated. The phenomena have been chosen and described for the North American continent, and especially for the United States. The basic data have been taken principally from the *American Ephemeris and Nautical Almanac*.

Sun. The sun in its northward motion will cross the equator on March 21, at 5 A.M., C.S.T., at which time spring will begin.

Moon. The phases of the moon will occur as follows:

		C.S.T.
Full Moon	March 6	9 P.M.
Last Quarter	14	12 Noon
New Moon	22	11 A.M.
First Quarter	29	10 A.M.

Evening and Morning Stars. Saturn will be the only conspicuous evening star; it will be well past opposition. Venus and Jupiter will continue to be brilliant morning objects; Jupiter will be past quadrature. Mercury and Mars will

remain too close to the sun for observation.

Mercury. Mercury will start the month moving westward toward the sun, which it will meet in inferior conjunction on March 8. After that date it will continue to move westward until March 21, when it will be stationary. It will end the month moving eastward more slowly than the sun, so that western elongation will not occur until early April. Perhaps it will be visible low in the dawn for the last few days of the month.

Venus. Venus will continue slowly to overtake the sun, but will remain a brilliant morning object.

Mars. Mars will continue slowly to move out into the morning sky, but will still be too closely to the sun for observation.

Jupiter. Jupiter will be nearly stationary in Libra. It will be in position for telescopic observation after midnight. The third of the series of monthly occultations by the moon will occur on March 12, but it will be visible only in the southern hemisphere.

Saturn. Saturn will continue slowly to retrograde in Cancer. It will be in an excellent position for evening telescopic study.

Uranus. Uranus will be slowly moving eastward in the eastern part of Taurus. It will be in quadrature on March 9, and so will be on the meridian at sunset.

Neptune. Neptune will be slowly retrograding in Virgo. It will be in opposition on March 30.

Students' Observatory, University of California, Berkeley, January 17, 1947.

Occultation Predictions for March, 1947

The quantities in the columns *a* and *b* are given for the purpose of making these predictions useful for any place within 200 miles of the point indicated. The procedure is as follows: Subtract the longitude of the point given from the longitude of the place in question; multiply the result in degrees, taking the signs into account, by the quantity under *a* for the star to be observed; similarly, with the latitude, using *b*; apply the sum of the products, with its proper sign, to the Greenwich C.T., and obtain the predicted Greenwich Civil Time for the phenomenon at the place of observation. To obtain Eastern Standard Time it is necessary to subtract five hours; Central Standard Time, six hours, etc.

(Taken from the *American Ephemeris*)

Date 1947	Star	Mag.	IMMERSION Greenwich C.T.	*a*	*b*	Angle E from N	EMERSION Greenwich C.T.	*a*	*b*	Angle E from N
			h m	m	m	°	h m	m	m	°
OCCULTATIONS VISIBLE IN LONGITUDE +72° 30′, LATITUDE +42° 30′										
Mar. 3	*A* Gemi	5.1	3 18.4	−1.5	−1.0	96	4 30.0	−0.9	−1.5	287
29	40 Gemi	6.3	23 52.7	..	..	21	0 15.2	..	..	348
OCCULTATIONS VISIBLE IN LONGITUDE +91° 0′, LATITUDE +40° 0′										
Mar. 3	*A* Gemi	5.1	2 49.7	−1.9	−1.3	118	4 5.2	−1.9	−0.1	259
12	41 Libr	5.5	11 59.1	−2.7	+0.4	57	12 48.4	−1.4	−3.0	348
29	40 Gemi	6.3	23 7.2	−1.3	+3.7	36	23 56.2	−2.2	−2.9	322

Date 1947	Star	Mag.	IMMERSION Greenwich C.T.	*a*	*b*	Angle E from N	EMERSION Greenwich C.T.	*a*	*b*	Angle E from N
			h m	m	m	°	h m	m	m	°
OCCULTATIONS VISIBLE IN LONGITUDE +120° 0′, LATITUDE +36° 0′										
Mar. 3	*A* Gemi	5.1	1 56.3	—1.8	—0.5	123	3 0.0	—1.6	+2.4	235
3	176 B.Gemi	6.3	9 52.0	+0.5	—2.7	154	10 29.0	—0.7	—0.1	235
12	41 Libr	5.5	10 44.0	—1.7	—0.1	114	12 11.1	—1.8	—1.0	308
12	κ Libr	5.0	13 7.3	—2.3	—0.9	108	14 36.8	—1.8	—1.6	300
14	θ Ophi	3.4	11 42.6	—3.0	+2.5	57	12 36.1	—0.6	—2.0	342
OCCULTATIONS VISIBLE IN LONGITUDE +98° 0′, LATITUDE +30° 0′*										
Mar. 12	41 Libr	5.5	11 39.1	—2.9	—0.3	89	13 3.3	—1.9	—2.3	320
14	θ Ophi	3.4	12 59.5	..	..	22	13 17.7	..	..	0
29	40 Gemi	6.3	22 33.5	—1.5	+1.6	70	23 52.9	—2.4	—0.1	281

*Computed by Edgar W. Woolard and Paul Herget; communicated by Commodore J. F. Hellweg, Superintendent U. S. Naval Observatory.

Asteroid Notes

By HUGH S. RICE

Because of paucity of ephemerides at present, we can give no actual minor-planet data at this time. It is a prodigious job to make up the yearly ephemerides of all the known planets, as was done formerly by the Coppernicus-Institut. Not only is it necessary to have the ephemerides computed, but also to keep them up to date—which in this case means to keep them accurate—observations of the planets are needed continuously, in order to improve the orbital data; so that there is a tremendous amount of unseen labor involved. For each observation of every asteroid, the "resids" are computed—the "O - C" or observation minus computation—which means the discrepancy between actual observed positions (the reduction of which is no simple matter) and the last computed or predicted positions of the planets. These "resids" assist in altering the orbital data and hence in improving future ephemerides. We believe the very best ephemerides are those of planet no. 4, VESTA: they are quite exact, being based on the famous Leveau's tables.

According to Dr. L. E. Cunningham, 1947 ephemerides are being prepared by several institutions in coöperation. Dr. Cunningham himself is computing the data for quite a number of planets.

We have received a few ephemerides from Dr. Dirk Brouwer of Yale. The dates of the positions do not begin until March, but beginning with the next issue the Notes will carry some data and ephemerides probably for each month during the remainder of the year.

Hayden Planetarium, American Museum of Natural History,
New York 24, N. Y., January 23, 1947.

Contributions of The Meteoritical Society

(Known Formerly as The Society for Research on Meteorites)

Edited by FREDERICK C. LEONARD
Department of Astronomy, University of California, Los Angeles 24

Hypothetical Meteorites of Sedimentary Origin*

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ABSTRACT

In 1939, Dr. Assar Hadding, the Director of the Geological Institute in Lund, Sweden, described two specimens (one of limestone and one of sandstone) that he believed to be of cosmic origin. This paper briefly reviews Hadding's account and discusses three other specimens, found in the United States, that may deserve consideration in connection with the specimens found in Sweden.

One of the most momentous discoveries that could be made in the field of meteoritics would be the positive identification of a meteorite of sedimentary origin. It would offer the first tangible evidence of the existence, at least in times past, of conditions favorable to life beyond the confines of the Earth. If the specimen were composed of limestone containing distinguishable shell fragments, the proof of extraterrestrial life would be undebatable; it would prove that certain meteorites, if not all of them, are products of the disintegration of a planetary body (or possibly several such bodies) having a hydrosphere as well as a lithosphere.

In December, 1939, Dr. Assar Hadding,[1] the Director of the Geological Institute at Lund, Sweden, presented a paper before the Swedish Physiographical Society, supporting the planetary origin of meteorites and describing two specimens—one of limestone and one of sandstone—that he believed had fallen from the sky. He related, with regard to the limestone specimen, that on Easter eve, April 11, 1925, "a beautiful meteor was observed moving towards the west across Östergötland and the Baltic outside." The next day he was informed that the stone had fallen "near the farm of Bleckenstad, just south of Mjölby." Unable to go there himself, he immediately sent one of the assistants of the Institute, Dr. Sven Holgersson, who returned two days later, bringing a sample of the "meteorite" and a report of his investigation at the site where it was found. The fall had been observed by several persons. "One of these, farmer Oskar Gustafsson," Hadding stated, "had at once collected bits of the crumpled stone and given them to Holgersson. The ground around the place where the stone had fallen was carefully examined in search of other stones that had more similarity to known meteorites. No such were found, but only a small heap of fragments from a stone of exactly the same kind as the one found by Gustafsson. The fields around the place also were searched, and more fragments were found. They all lay in a narrow belt along the course that the stone was said to have taken. . . . .

"Oskar Gustafsson, a respected and reliable man, whose words could not be doubted, had related that he had seen the bright body in the sky and that it was

*Read at the Ninth Meeting of the Society, 1946 September.

seen to fall. He was standing on the road leading up to his farm, and, in the field in front of him, some 50 meters from the road, two children were playing, his niece and nephew. Suddenly he saw the falling body sweeping over the heads of the children like a white ball and breaking against the ground. 'It looked like a newspaper that had been crumpled up into a ball. Somehow it fluttered open.' The children were scared out of their wits; the boy fell onto the ground and then rushed to his uncle crying, 'The Moon is falling down! We must go home!' Gustafsson had taken the children up to the house and then gone back in order to find out what had fallen down. He found the split-up, white stone and knew at once that it was limestone. He collected some of the splinters, finding the whole thing rather curious. One thing he was convinced of. Nobody could have thrown the stone there, and, in the 25 years that he had owned the farm, he had not limed the land a single time. He was absolutely certain that the stone must have fallen from the sky."

After Holgersson returned to Lund, Hadding sought a supplementary report from Dr. Sven Zensen, of the Swedish Riksmuseum, who also had gone to Bleckenstad to investigate the fall. Apparently, Zensen found nothing either to strengthen or to discredit Holgersson's report, for Hadding observed that "there were no essential new points." All the fragments of the stone found by Gustafsson and Holgersson "were lying loosely on top of the crop and the grass in the field," Hadding stated. "They must have only just got there." In conclusion, he expressed the conviction that, "under the circumstances," the observations on the fall "could hardly have been better. The stone, or rather the splinters that were preserved, were, according to Gustafsson's account and Holgersson's investigations, the very stone that fell."

This narrative, which is quoted from an English translation published in Sweden, is followed by a description of the specimen: "a white, or slightly grayish-yellow, fine-grained mass, somewhat porous," composed of almost pure calcium carbonate. Small fragments of calcareous shell, undefinable in character, are scattered thru it. Among Swedish rocks, Hadding stated, he knew of none that bore a striking resemblance to the specimen. "It is impossible to mistake the splinters for any of them; nor can they be mistaken for burned or slaked lime from a sugar refinery, or any other product to be found in the country." The breakage of the stone, which reduced it to what Hadding called splinters, made impossible any satisfactory determination of its surface features. Some of the splinters, however, revealed a peculiar gloss, according to his statement, which he had never seen "in natural limestones or in burnt or otherwise prepared stones." He believed that the gloss may have resulted from atmospheric friction during the flight of the stone, which he designated as the "Bleckenstad meteorite."

The sandstone specimen received much less attention in Hadding's paper. "Several years ago," he stated, "I was sent, from a farm in South Sweden, a small sample of stone that was said to be a meteorite. A farmer had been walking with his wife and daughter in the garden one morning when suddenly a stone came spinning thru the air. Nobody had thrown it. It was crumpled against the garden path, but the fragments were collected and sent to me. It was loose sandstone. In spite of the assurance of the absolutely trustworthy finders that the stone was a meteorite, I presumed some kind of mistake, and the fragments were not preserved for further investigation. Alas! Others may have acted like me!"

I believe that the publication of reports such as Hadding's serves a valuable

purpose, especially if the reporters use care in discriminating between credible evidence and presumption. Meteoritics is a science that can be advanced best thru a liberal exchange of information that may possibly establish the existence of meteorites of types now unknown. It should not be necessary for a researcher to withhold a description of a puzzling specimen until he feels prepared to commit himself to a positive identification of it. The decision to publish should be based upon a consideration of the value of the report to other researchers whose observations it may supplement.

Among the thousands of specimens (including many of unquestionably cosmic origin) that were sent to me by correspondents in all parts of the country, as a result of my numerous magazine articles on meteorites in the period between 1934 and 1941, were two specimens that may conceivably add at least a footnote to Dr. Hadding's observations. A third specimen, which will receive my attention first, was offered for my study several years ago by Dr. H. H. Nininger. It was discovered in northern New Mexico in the search for fragments of the Pasamonte Ranch fall of March 24, 1933, which it superficially resembles. This specimen has roughly the form of a sphere-quadrant with a radius of about 3 cm. Approximately two-thirds of the surface exhibits a thin fusion crust that may be assumed to have covered all of it before the stone was broken. Except for a narrow band of brownish-gray, which borders about 3 cm. of the broken surface, the crust is black, as a result of the inclusion of free carbon. The interior, revealed by the fracture, is a porous, dirty-gray limestone, composed of fragmentary shells. I have been unable to observe any of the shell forms that appear to me to be identifiable. It is the fusion crust that seems to me to make the specimen worthy of attention. I am aware, of course, that the presence of such a crust is not sufficient evidence to justify the identification of the specimen as a meteorite. On the other hand, the formation of a fusion crust on a specimen of limestone by processes not related to the fall of a meteorite is not an easy phenomenon to explain. Limestone does not fuse directly under heating. It is first reduced to CaO, which fuses at 2572° C., a temperature difficultly achieved by artificial means. Quartz, by contrast, fuses at temperatures from 1470° C. upward. It is difficult to understand how any small mass of limestone could be heated intensely enough to fuse a crust on it without reducing, or calcining, the entire specimen. Then, on exposure to atmospheric moisture, it would unite with H_2O to form calcium hydroxide, which would eventually revert to limestone by absorbing carbon dioxide from the air. Any limestone specimen, under ordinary circumstances, might be expected to disintegrate before these processes were completed. After calcination, if it remained intact, even a moderate disturbance would normally reduce it to a powder. The next hazard would be slaking, which would result from its first contact with water. These hazards would be circumvented, however, if the surface fusion were, in some manner, accomplished before the heat could penetrate into the interior of the specimen. Such heat is developed in the flight of a meteorite. It would seem, moreover, that the fusion must have been almost instantaneous to free the carbon from the carbonate and to involve it in the melt before it could escape as an oxide. An artificial origin of the fusion crust seems to me to be difficult to accept. Even if it could have been artificially produced, the discovery of the specimen in a barren wasteland in northern New Mexico would still be puzzling. Certainly it could not have been formed by volcanic action, and the possibility is hardly worth considering that it could have been

formed by lightning.

The other two specimens—the two that were submitted to me directly by their finders—are composed of grayish, fine-grained sandstone. One was sent to me in 1940 by Mr. Leonard Fishel of Trevlac, Brown County, Indiana, who stated that he had found it in a pasture, remote from any source that might have suggested an artificial origin for the thin, greenish, glass crust that encompasses it. The specimen puzzled me. Eventually, however, my interest lagged; I returned it to Mr. Fishel and thought no more about it for several months. Then another specimen of remarkably similar appearance came to me from Miss Kathleen Auvil, of Montrose, Randolph County, West Virginia. Miss Auvil said that she had found the stone near the head of a small creek, "in a country very aptly described as the West Virginia hills." The site was 15 miles from Elkins, Randolph County, and 3 miles from the nearest village, and no traces of any pottery works or any other industry that might have accounted for the specimen were known to her anywhere in the vicinity. This discovery revived my interest and prompted me to request Mr. Fishel to submit his stone to me again for comparison and further examination. Both stones have now been in my possession for about 5 years.

Unfortunately, a considerable part of the Trevlac stone has been broken away and lost. A minor part of the breakage occurred before Mr. Fishel found the specimen; the rest was done by an experimenter who volunteered to help me ascertain the character of the stone and who apparently failed to recognize that he was destroying valuable evidence! I had submitted it to him because he had been recommended to me as an authority on ceramics and the fire-clay industry, whose opinion would be helpful in probing the possibility that the glassy crust

FIGURE 1

AN "AMATHOSITE" FROM TREVLAC, INDIANA

FIGURE 2

AN "AMATHOSITE" FROM MONTROSE, WEST VIRGINIA

on the specimen had been formed in a kiln. He was very doubtful that it could have been the product of any fire-clay operation. The portion of the Trevlac specimen that remains unbroken is roughly a quadrant of a sphere, or a triangular pyramid with one face broadly rounded. It has a maximum length of slightly more than 8 cm. Aside from the fusion crust, the surface features of the specimen offer no strong evidence either to support or to discount the supposition that it may have been subjected to atmospheric erosion. It exhibits no piezoglyphs and no flow lines, nor any pits or notable depressions of the type observable on many stones of cosmic origin; on the other hand, it can be fitted readily into the range of forms exhibited by stony meteorites that have not maintained a fixed orientation during their flight. The transparent crust of greenish glass that covers virtually the entire unbroken surface of the stone appears to average about ½ mm. in thickness, a measurement wholly compatible with the hypothesis that it may have been formed by the heat of atmospheric friction. A network of cracks in the glass indicates that it cooled rapidly after exposure to the heat that formed it, and in several places small areas of the surface appear to have sloughed off before the heating ceased. These areas show an incipient fusion such as may sometimes be observed on meteorites that have broken in flight. If the fusion of the stone proceeded far enough at any point to penetrate more than a fraction of a millimeter below its surface, no accumulations of glass remain to bear evidence of the process. Either the fusion was halted before it penetrated farther or the plastic material was removed almost as rapidly as it was formed. I am hesitant to say that the stone shows evidence that it has been sculptured to some extent by external fusion; yet the shallow, irregular undulations of the surface and the rounded edges definitely suggest such a possibility. The most remarkable feature of the specimen is, however, not the fusion crust. It is a crack that penetrated into the stone about 29 mm.,—that is, before my collaborator of the ceramics industry chipped away one face of it! Fortunately, the other face still remains, and probably enough of the crack is still accessible to examination to confirm my description of it as it originally appeared when Mr. Fishel submitted the specimen to me. On both faces of the crack the stone was completely covered by a film of glass which may still be observed on the face that remains unbroken—a film so minutely thin that it follows, in some degree, the contour of the sand grains beneath it. There is no checking or cracking of the glass. This feature, as well as the thinness of it, differentiates it from the glass that covers the exterior of the specimen. When the stone came to me, the deeper part of the crack contained a considerable amount of crushrock, or myolinite, fused barely to the point of making the particles adhere together and to the glassy faces. The evidence of crushing was unmistakable. An almost identical appearance may be obtained by placing a bit of sand between two iron plates and grinding them together. Still deeper in the stone and still observable, the crack contracts into a minute glassy vein that continues for slightly more than 1 cm. I believe that the evidence is conclusive that the two opposing films of glass were formed in the crack. The hypothesis is untenable, in my opinion, that the glass could have entered the crack from outside to coat the two faces of it without flowing together. An equal amount of molten glass introduced from the outside could have proceeded only a few millimeters before congealing, instead of spreading uniformly over the two faces in an almost microscopically thin film and penetrating to the deepest recesses of the crack to form a minute vein of glass. The presence of the crushrock seems to me to point

unmistakably to the conclusion that the glass in the crack was formed by faulting under high pressure.

Small dark veins are very common in meteorites. Dr. Virgil E. Barnes,[2] of the University of Texas, who has made a study of them, states that in composition they resemble the fusion crusts of the meteorites in which they appear. He believes that they were formed by faulting. In *The University of Texas Publication No. 3945*,[2] he describes a meteorite found near Cuero, Texas, which exhibits cracks and veinlets that seem to correspond, in some degree, with the crack and vein in the Trevlac stone. "Surfaces of the crack," Dr. Barnes states, "have well-developed slickensides along them, indicating that they are fault surfaces. . . The filled portion of the cracks is probably a myolinite which is coherent enough to hold the meteorite together." Veins similar to the veins in meteorites have been found in terrestrial formations, Barnes states, but "only in metamorphosed rocks, and [they] must have been formed at depths of several miles below the surface and consequently under rather high pressures." No terrestrial sandstone is known, of course, to exist at such depths. As one possible origin for the veins in meteorites, he suggests that they may have been caused by the "disaggregation of a celestial body during a close approach or collision."

The standstone that composes the Trevlac stone is extremely compact and has been partially metamorphosed by heat. It will even take a degree of polish with a fine emery cloth. The evidence is strong, however, that the partial metamorphosis did not result during the same application of heat that formed the fusion crust. There is no gradation in the degree of metamorphosis from directly under the fusion crust to the center of the specimen. It is fully uniform.

E. P. Henderson and Harry T. Davis[3] have described a stony meteorite from Moore County, North Carolina, that reveals "very definite evidence of reheating" before it entered the atmosphere of the Earth. They do not hypothesize on the origin of the heat, except to comment that it was likely applied at the place where the meteorite originated rather than during a close approach to the Sun. An obvious possibility is that it may have been generated by the disintegration of a planetary body in some cosmic catastrophe. According to the authors of the description, the heat that caused the alteration in the specimen "was somewhere between 1090° . . . 1510° C." A similar degree of heat would probably have caused the partial metamorphosis observed in the Trevlac stone.

The Montrose stone, while it shows a remarkable resemblance to the Trevlac stone, exhibits no feature of comparable interest to the crack in the other specimen. It is roughly a truncated, quadrilateral pyramid, with a maximum altitude of about 9 cm. and a base averaging about 5×6 cm. The glass crust that envelops most of it is greener and also slightly thicker than the crust of the Trevlac stone. Moreover, it shows less uniformity in various ways. Deeper shades of green in some places indicate some thickening of the glass, especially on one side, where the irregularity of the surface suggests considerable sculpturing as a result of material fused away. In two spots, more especially, the glass has blistered and broken. The principal departure from uniformity is, however, on another side where the fusion has resulted in forming a rather extensive area of slaggy glass, filled with gas cavities and unfused grains of sand. The interior of the stone, like the interior of the Trevlac stone, shows evidence of uniform heating. It is evident likewise that the heat that affected the interior of the stone was not the same heat that formed the fusion crust. If the heat that fused the surface

had been continued long enough to penetrate to the center of the stone and to cause a partial alteration of the sand grains there, and if no force had been present to remove the molten material from the surface almost as fast as it fused, considerable accumulations of glass on the outside would surely have resulted. A peculiar feature of the interior of the stone is observable on the basal cross-section, which has been ground flat to facilitate examination, as a border of darker sand grains flanking two sides of the quadrilateral to a depth of about 1½ cm. Toward the center of the stone several shadowy filaments diverge from the border, giving it the appearance of a spray. The border and the filaments attached to it exhibit no similarity to the concentric bands to be seen in concretionary formations.

Dr. Hadding makes the logical point that sedimentary meteorites, if they do exist, are bound to be extremely rare in comparison with meteorites of the heavier types. As the heavy magma of the Earth far outbulks the sedimentary and eruptive layer that envelops it, so would the heavy nuclear materials compose the greater part of any planet that may have disintegrated into cosmic rubble. Hadding reasonably remarks also that "we should consider a stone that looked like granite or some other known rock not as one of cosmic but as one of tellurian origin." It would be hard to identify as a meteorite and it would be even harder to gain its acceptance as one!

I wish to emphasize, in conclusion, that the purpose of my discussion has not been to argue that sedimentary meteorites do, of a certainty, exist; rather, it has been to set down certain observations that may conceivably be helpful to other researchers who may become interested in exploring the possibility of their existence. If sedimentary meteorites do exist, they should be grouped and named to distinguish them from other types of cosmic missiles already known. Even while they remain in the realm of speculation, some designation will facilitate discussion of them. I suggest, therefore, that the sandstone variety be called *amathosites* and that the limestone variety be known as *calcarites*.

References

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[2] Barnes, Virgil E., *Univ. of Texas Publ. No. 3945*, 645-54, 1940.

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The Smithonia, Oglethorpe Co., Georgia, Siderite (0832,340)

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Late in 1941 and early in 1942, I had some correspondence with Mr. Corbett Simmons of Elberton, Georgia, regarding an iron meteorite (a siderite) that was then in his possession; he sent me some fragments, weighing about 25 grams, and also a couple of photographs (*v.* Figs. 1 and 2) of the specimen; and he supplied me with part of the following information concerning it.

Altho the meteorite was originally discovered early in 1940 by another person (unnamed), Mr. Simmons found it on May 23, 1941. The specimen, a complete individual, measured 19½ × 11 × 7 inches, and weighed 154 pounds. From the photographs—I have never seen the meteorite itself—I would describe it

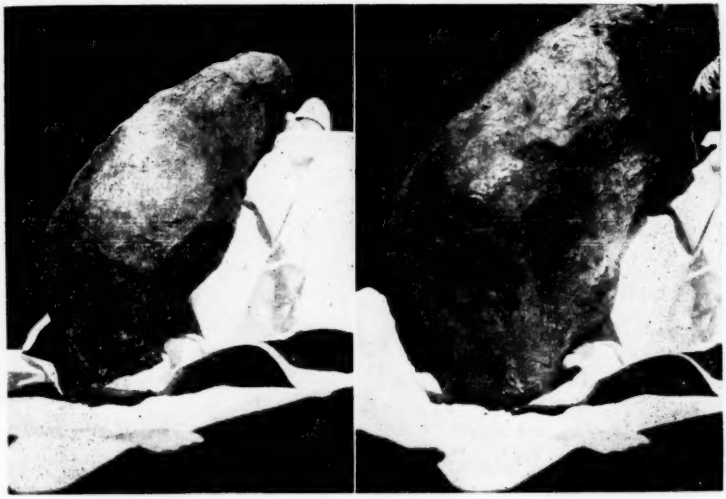

FIGURE 1 FIGURE 2

TWO VIEWS OF THE SMITHONIA, OGLETHORPE CO., GEORGIA, SIDERITE

(Photographs by Corbett Simmons)

as a roughly thigh- or ham-shaped mass and probably a very old fall. The place of find was 23° south of west of Elberton, Elbert Co., and 23 miles thence, in an air line. Presumably the nearest post office being Smithonia, Oglethorpe Co., the name of that town has been given to the meteorite. From the foregoing specification of the place of find, it follows that the coördinate number of the siderite is 0832,340.

The Smithonia meteorite was sold by Mr. Simmons to the Chicago Natural History Museum, where it is now preserved and where it has been classified as a hexahedrite (H).

1946 December 31

The Barringer Meteorite Crater (Abstract)*

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This paper contains a discussion of the various hypotheses regarding the location and present condition of the meteoritic material that produced the Barringer Meteorite Crater near Canyon Diablo, Arizona.

References and comparisons are made to the Haviland, Kansas, and Odessa, Texas, Craters, in regard to the wide range of destructive effects of meteoritic impacts, with the conclusion that a liberal sprinkling of meteoritic fragments probably exists on the true floor of the Barringer Crater and that a congestion

*Read by title at the Ninth Meeting of the Society, 1946 September.

of large masses may lie under the south wall and in the southwestern quarter of the pit.

Distribution beyond the Crater rim and on the outlying terrane is discussed, on the basis of a survey made in 1939 that revealed a pattern formed by a scattering of masses outward from the pit.

It is concluded that the lack of evidence of intense heat having been formed at the time of the impact, comparable to that in the cases of the Henbury, Australia, and Wabar, Arabia, Craters, reveals that the meteorite or meteorites must have struck the Earth at a comparatively low velocity, thereby producing a relatively mild explosion, and that a considerable portion of the original mass still remains in the pit as fragments.

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The Observations of Giacobinids in the U.S.S.R.*

Observations were made mainly in Moscow, as the sky was cloudly in most parts of European Russia and already daylight in middle Asia. In Moscow the Commission on Meteors, Comets, and Asteroids of the Academy of Sciences of the U.S.S.R. organized astronomic, ionospheric, magnetic, and radio observations and also observations with radar. An aerostat was flown in case of a cloudy sky. Its passenger, Katasev, counted the number of meteors, Belousov, observed conditions of short-wave radio communication.

According to observations made by the members of the Sternberg State Astronomical Institute, students of the Moscow State University (12 observers, instructress Bugoslavskaya) and members of the Moscow branch of the All-Union Astronomo-Geodetical Society (7 observers, instructor Bronsten) the number of meteors in the whole sky was as follows:

October	9	23^h - 24^h	U.T.	6 meteors per 10 minutes
	10	0 - 1	18	"
	10	1 - 2	38	"
	10	2 - $2^h\,30^m$	(58)	"

The last number is underestimated 1.5—2 times because of the appearance of clouds and the dawn. Observations with radar showed that maximum was at about $3^h\,50^m$ U.T., the rate being 20 times larger than that at 2^h U.T.

I. A. Astapovitch, who carried on observations at Leningrad, estimated that at about 3^h U.T. there were 120 meteors per 10 minutes. He received the following uncorrected radiant $\alpha = 258°$, $\delta = 53°$ (1945.0), $n = 69$. N. Sitinskaya (Leningrad) received the following coordinates for the radiant (corrected for zenith attraction): $\alpha = 264°.7$, $\delta = 53°.5$ (1946.8), $n = 11$. The stationary meteor observed by Chernishev (Moscow) gave the corrected position of the radiant as being $\alpha = 264°.5$, $\delta = 52°.9$ (1946.0).

As the number of meteors became sufficiently large just before the dawn only three meteors were photographed. Observations with radar organized by

*Report sent by Dr. B. Levin to Dr. Fred L. Whipple and transmitted by him to POPULAR ASTRONOMY.

Levin and Checkick showed that most reflections of meteor trails lasted 0.5—1 second; in some cases up to 35 seconds.

Ionospheric observations made by the Institute of Physics of the Academy of Sciences of the U.S.S.R. (Alpert), the Institute of Earth Magnetism (Pushkov, Mednikova) and the Ionospheric Stations of the Institute of Communications (Bulatov, Kosikov) showed that at about 23^h U.T. (October 9) reflections from the sporadic E layer appeared. Observations made by stations having an automatic registering device showed that the height of reflections instead of the ordinary 100 km., equalled about 70 km., *i.e.*, coincided with the height of the meteors. The critical frequency of the sporadic E layer increased and at $4^h 30^m$ U.T. reached 9 megacycles, thereby exceeding the critical frequency of the F_2 layer.

No influence of meteors and this sporadic layer on the absorption of radio-waves in the ionosphere and on radio reception was noticed. The magnetograms received by the Institute of Theoretical Geophysics of the Academy of Sciences (Groshevoj, Kolubakin) and the Institute of Earth Magnetism showed deviations at about 23^h U.T. (October 9) and at about 9^h U.T. (October 10). However their relation with the passage of the earth through the meteor stream has yet to be checked.

VARIABLE STARS

Variable Star Notes from the American Association of Variable Star Observers

By LEON CAMPBELL, Recorder

Observations of SS Cygni: In the October, 1945, issue of these Notes, the observations of SS Cygni are plotted from J.D. 2430700 to 1700. The tabulated and plotted observations from J.D. 2431700 to 2432185—the end of 1946—are given herewith. The total number of estimates made during these 16 months is 1,493, or an average of about 3 per day. Observations were obtained on 389 of the 485 days included in this discussion.

In the first table are given the daily means, with the number of observations used by each mean. In the second table are listed the nine maxima observed during this interval of time, with the data pertaining to type, times of reaching magnitude 10.0 increasing, maximum, and magnitude 10.0 decreasing, with the difference in time between each.

As will be noted, the intervals between the successive times of maximum vary from 29 to 78 days, with an average value of 52.6 days. Maxima numbers 350 and 351 are not readily classified since they are decidedly anomalous. Maximum 351 barely attained magnitude 10.0.

TABLE I

DAILY MEANS

213843 SS CYGNI J.D. 2431700 - 2190

243	—0—	—1—	—2—	—3—	—4—	—5—	—6—	—7—	—8—	—9—
1700	3 11.80	7 11.89	11 11.86	11 11.78	8 11.28	11 9.11	7 8.73	10 8.75	5 8.88	8 9.04
10	6 9.20	8 9.58	5 10.34	.....	6 11.28	.....	1 11.7	.....	3 11.80	.....
20	4 11.88	4 11.80	2 11.65	2 11.65	6 11.82	6 11.82	3 11.77	2 11.80	1 11.7	5 11.18
30	1 8.9	9 8.62	10 8.76	5 8.66	1 8.8	3 8.57	9 8.47	5 8.62	5 8.74	5 8.78
40	4 8.78	7 8.93	3 9.07	.....	1 9.4	7 9.90	4 9.85	1 10.7	1 11.2	1 11.1
50	1 11.3	.....	.....	.....	1 11.8	5 11.84	5 11.74	11 11.62	5 11.68	5 11.78
60	4 11.58	11 11.84	1 11.6	4 11.70	.....	10 11.77	4 11.78	3 11.77	1 11.9	3 11.90
70	2 11.85	1 11.8	.....	1 12.0	1 12.0	8 11.65	1 11.8	.....	6 8.82	1 8.7
80	3 8.93	4 8.90	3 9.33	3 9.70	3 9.67	6 10.68	8 10.89	4 11.20	1 11.7	3 11.30
90	2 11.20	4 11.62	3 11.87	1 11.9	6 12.00	3 11.93	1 11.8	2 11.90	3 11.97	.....
1800	1 12.0	2 12.10	2 11.85	2 11.85	2 11.90	.....	.....	.....	1 12.1	.....
10	1 11.8	2 11.95	3 11.93	1 11.8	2 11.15	1 10.2	.....	7 9.93	6 9.68	1 10.0
20	1 9.3	3 9.30	8 9.08	8 8.90	4 9.08	4 9.25	5 9.48	4 9.65	2 10.00	7 10.56
30	3 11.10	2 11.50	2 11.90	.....	2 11.90	.....	.....	.....	.....	3 11.90
40	1 12.0	.....	2 12.00	3 11.87	2 11.40	1 10.6	1 10.4	2 10.25	4 10.02	3 10.47
50	3 10.80	2 11.60	.....	2 11.70	2 11.85	1 11.6	2 11.70	.....	.....	2 11.75
60	1 11.9	.....	1 11.8	1 11.8	1 11.9	.....	.....	.....	1 11.4	1 12.0
70	.....	.....	1 12.0	1 11.9	.....	.....	.....	.....	.....	.....
80	.....	.....	1 11.9	.....	.....	.....	1 11.8	1 11.6	.....	.....
90	.....	1 11.9	.....	.....	.....	.....	.....	.....	.....	1 12.0

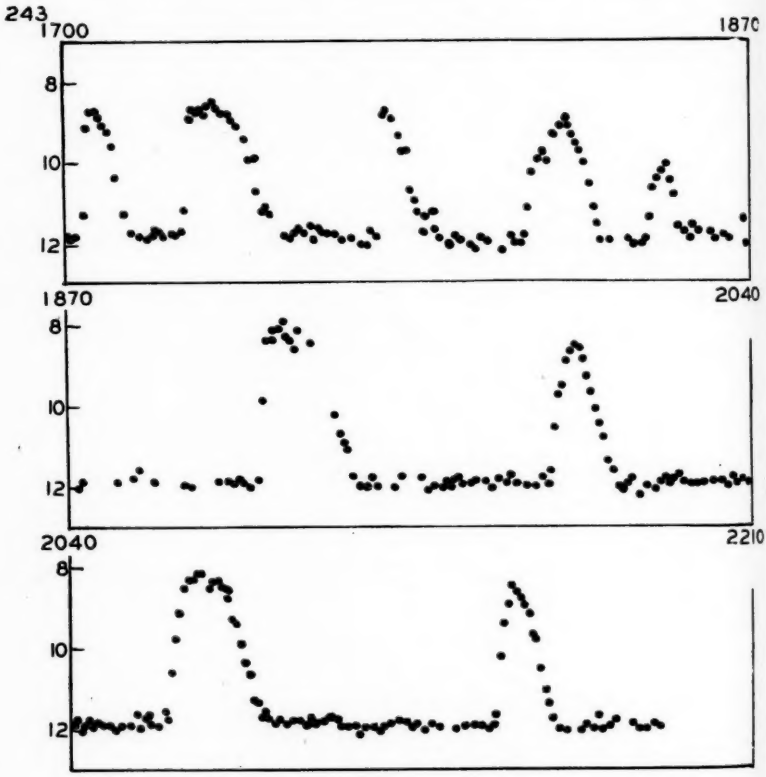
TABLE I (CONTINUED)

243	—0—	—1—	—2—	—3—	—4—	—5—	—6—	—7—	—8—	—9—
1900	1 12.0	.....	.....	.....	.....	.....	.....	1 11.9	.....	.....
10	2 11.90	2 11.90	1 11.8	1 11.9	2 12.00	2 12.00	.....	4 11.82	1 9.8	2 8.40
20	2 8.40	3 8.17	1 8.1	1 7.9	3 8.23	2 8.40	1 8.6	2 8.15	.....	.....
30	.....	1 8.4	.....	.....	.....	.....	1 10.2	1 10.7	1 10.9	2 11.05
40	2 11.75	.....	1 12.0	.....	1 12.0	1 11.8	1 12.0	.....	.....	.....
50	1 12.0	.....	1 11.8	.....	.....	.....	.....	1 11.8	.....	1 12.1
60	1 12.0	.....	1 12.0	2 11.90	2 12.00	2 11.85	1 11.8	2 11.95	2 11.90	5 11.92
70	5 11.86	2 11.95	.....	1 11.9	4 12.05	3 12.00	3 11.83	4 11.82	2 11.90	1 11.7
80	4 11.95	3 11.90	.....	2 12.00	1 12.0	1 12.0	1 11.8	3 11.80	1 11.9	1 11.6
90	6 10.52	5 9.74	3 9.50	6 8.92	11 8.65	11 8.55	13 8.58	13 8.86	9 9.29	11 9.66
2000	9 10.07	10 10.47	8 10.78	6 11.35	11 11.56	6 11.60	5 12.00	3 12.07	2 11.90	2 11.80
10	.....	1 12.2	.....	1 12.0	.....	3 12.07	1 11.9	6 11.82	6 11.93	9 11.84
20	7 11.87	2 11.70	1 11.9	5 11.94	6 11.95	4 11.98	8 11.98	3 11.97	8 11.91	9 11.91
30	8 11.89	11 11.88	4 11.95	3 12.00	1 11.8	5 11.98	6 11.93	3 11.87	3 11.87	4 11.95
40	3 11.93	2 11.75	2 12.05	2 11.90	4 11.80	1 11.9	1 11.8	3 11.93	3 11.87	3 11.90
50	2 11.90	8 12.00	6 11.93	7 11.97	1 11.9	4 11.90	6 11.63	7 11.97	6 11.73	4 11.85
60	4 11.90	3 11.93	4 11.85	9 11.53	9 11.70	12 10.60	10 9.80	12 9.03	9 8.51	9 8.29
70	5 8.22	5 8.14	2 8.15	1 8.3	3 8.47	7 8.30	4 8.30	9 8.43	7 8.50	8 8.75
80	13 9.25	7 9.31	6 9.87	8 10.30	8 10.61	3 11.30	4 11.38	4 11.68	3 11.57	7 11.70
90	9 11.84	6 11.77	4 11.72	2 11.85	2 11.90	3 11.73	2 11.70	5 11.82	7 11.86	2 11.70
2100	4 11.82	1 11.9	1 11.8	.....	.....	.....	1 11.8	2 11.85	7 11.93	3 11.97
10	4 11.90	1 12.1	9 11.98	5 11.98	5 12.00	6 11.97	6 12.03	7 11.98	5 11.92	6 11.87
20	3 11.80	4 11.80	3 11.83	4 11.82	8 11.95	2 11.90	.....	1 12.0	.....	4 11.90
30	6 11.95	6 11.92	.....	.....	.....	2 12.00	.....	8 11.98	8 11.95	4 11.95
40	9 11.94	5 11.92	2 11.85	7 11.96	8 11.94	8 11.72	6 10.22	5 9.40	6 8.98	4 8.42
50	6 8.57	4 8.78	10 8.88	4 9.18	9 9.70	4 9.78	5 10.49	5 10.64	1 11.4	1 11.8
60	.....	.....	1 12.0	.....	.....	.....	2 12.00	1 11.9	4 11.88	1 12.0
70	3 11.73	2 12.00	.....	2 11.95	1 11.8	.....	.....	.....	.....	1 11.9
2180	1 12.0	.....	1 11.8	.....	2 11.90	2 11.90	.....	.....	.....	.....

TABLE II

OBSERVED MAXIMA

No. Max.	Type	J.D. 10.0 I	Diff.	J.D. Max.	Diff.	J.D. 10.0 D	Diff.
347	A4	2431705	30	2431707	31	2431712	31
348	A10	1730	25	1736	29	1746	34
349	A2	1778	48	1780	44	1784	38
350	?	1816	38	1824	44	1828	44
351	?	..	..	1848	44	..	..
352	A10	1918	..	1925	77	1935	..
353	A5	1991	73	1996	71	2000	65
354	A9	2066	75	2074	78	2083	83
355	B2	2432147	81	2432149	75	2432156	73



SS CYGNI 1945-46

The light curve is shown in the figure. A total of 355 maxima have been observed since the discovery of the variable by Miss L. D. Wells at Harvard in 1896. So far as can be determined, not a single maximum has escaped attention, having at least one observation made on some portion of the light curve while

above minimum light. Truly a remarkable record!

The R Coronae Borealis Variables: R Coronae Borealis itself is around magnitude 11.0, perhaps on its way up from minimum, which was reached late in November.

RY Sagittarii was thought to be fading to minimum a month or so ago, but, due to its low altitude for northern observers, there was some doubt. Observations recently received from far southern observers confirm the fact that the star began a decrease in light in November last, and had dropped to magnitude 8.5 at the end of that month.

Other observations from South Africa also indicate that S Apodis was on the wane, magnitude 10.8 on December 1.

We have here another instance of three of the R Coronae Borealis-type stars fading to a minimum at or about the same time!

More Novae in 1944: The discovery of novae by means of their typical spectral appearance has again proved fruitful at the hands of Mrs. Margaret L. Mayall of the Harvard Observatory. In a recent examination of spectrum plates taken with the 10-inch Metcalf telescope in South Africa in 1944, she has found two more novae, one in Sagittarius, R.A. $18^h\,1^m1$, Dec. $-33°\,22'$ (1900), and the other in Scorpius, R.A. $17^h\,16^m4$, Dec. $-35°\,49'$ (1900). The first one was not seen on a plate taken April 5, 1944 (below magnitude 16.5) but was of magnitude 8.0 on April 16 (J.D. 2431197), magnitude 13.5 on May 16 (J.D. 2431227), and magnitude 16.0 on August 6 (J.D. 2431309).

The nova in Scorpius was not seen (fainter than 16.5) on May 5, 1944, but on May 15 (J.D. 2431226) it was of magniutde 7.5, and on August 6 (J.D. 2431309) it had decreased to magnitude 13.4. Both novae would appear to be of the rapidly fading type, and more plates are under examination to determine the complete light curves.

Observations received during December: A total of 3,705 observations were received in December from 60 observers, as follows:

Observer	No. Var.	No. Ests.	Observer	No. Var.	No. Ests.
Adamopoulos	19	24	Halbach	18	19
Ahnert, F.	5	24	Harris	31	31
Ahnert, P.	38	223	Hartmann	172	196
Ball	10	10	Hukill	4	45
Bappu, M. K. V.	7	15	Jackson	1	1
Blunck	6	7	Katsis	23	85
Bogard	5	16	Kelley	5	19
Boone	14	14	Kelly	14	17
Buckstaff	14	26	Kilby	2	2
Chassapis	135	152	Kitley	29	44
Cilley	50	149	de Kock	93	370
Dafter	9	17	Koons	41	42
Damkoehler	5	27	Kotsakis	6	9
Elias	50	107	Luft	1	6
Fernald	315	594	Mallas	33	114
Fleischer	2	5	Marston	1	1
Flower	3	4	Matthews	10	216
Focas	26	36	Mount	3	12
Fredlund	1	2	Oheim	64	70
Garneau	5	5	Oravec	28	30

Observer	No. Var.	No. Ests.	Observer	No. Var.	No. Ests.
Pardon	4	4	Stachelek	1	1
Parks	17	17	Stahr	5	30
Peltier	141	191	Taboada	38	38
Plakidis	50	194	Thomas	9	17
Reeves	2	6	Wade	7	10
Robinson	10	15	Webb	16	17
Rosebrugh	31	165	Weber	20	20
Salem	1	1	Welker	22	37
Sherman	8	10	Zirin	52	78
Sill	45	45	—		—
Smith	4	23	60	totals	3705

January 15, 1947.

Comet Notes

By G. VAN BIESBROECK

Last month it was mentioned that COMET 1946 *h* (JONES) was coming in reach of northern observers in January. Already on January 3 H. Giclas, Lowell Observatory, Flagstaff, Arizona, secured an observation as follows:
1947 Jan. 3. 3.55537 U.T. $\alpha = 17^h\,33^m\,49^s.17$ $\delta = 10°\,51'\,15''.3$ Magnitude 10.2. This is the first position obtained from this side of the equator. The comet was discovered in the Southern Hemisphere August 6 as an object of magnitude 9. From an arc extending over three weeks L. E. Cunningham has computed a new orbit which gives the following prediction (*H. A. C.* 790):

1947	α h m	δ ° ′	Mag.
Jan. 31	18 34.0	— 1 06	9.9
Feb. 8	18 48.7	+ 1 50	10.2
16	19 2.2	4 48	10.5
24	14.7	7 49	10.8
Mar. 4	19 25.9	+10 51	11.1

The measure on January 3 indicates a small correction of $-0^m.7$ and $-5'$ in the coordinates. The comet, which is now on the decline, fully a magnitude fainter than the prediction, will therefore remain inconspicuous but the conditions of visibility will improve as the region moves away from the morning sky.

In the early hours of the night COMET 1946 *k* (BESTER) follows its predicted course in the constellation of Cetus. It is now past its brightest phase but will remain visible for some time longer. Improved elements by L. E. Cunningham (*H. A. C.* 788) give the ephemeris:

1947	α h m	δ ° ′	Mag.
Feb. 8	1 18.4	— 1 44	9.3
16	17.8	+ 1 11	9.4
24	18.2	3 57	9.5
Mar. 4	1 19.6	+ 6 34	9.6

On January 11 the writer estimated the total magnitude from visual out-of-focus comparisons with known stars as being $10^m.6$ on the Harvard Scale. This is about two magnitudes fainter than the prediction. The comet showed a well-condensed

but not stellar nucleus. The coma broadens into a diffuse extension toward the east suggesting a faint tail in that direction.

Periodic Comet Tempel II and Brooks are now too faint for further measures but both have been well observed at their 1946 return. Periodic Comet Schwassmann-Wachmann 1925 II is still very well located and should be watched for possible bright outbursts.

This spring the return of Periodic Comet Grigg-Skjellerup is once more expected. G. Merton (Oxford, England,) kindly sent the following emphemeris in advance of publication in the *Handbook of the British Astronomical Association:*

1947	α h m	δ ° ′
Feb. 8	6 29.0	—25 58
16	6 10.9	26 43
24	5 53.9	26 54
Mar. 4	37.9	26 36
12	22.0	25 57
20	5 03.3	24 54
28	4 36.7	—23 8

The comet moves across the constellation of Canis Major and brightens up considerably in March. Perihelion is expected 1947 April 20. The period is only 4.91 years.

Williams Bay, Wisconsin, 1947 January 13.

General Notes

Professor Harold Jeffreys has been elected to the Plumian professorship of astronomy and experimental philosophy in the University of Cambridge, in succession to the late Sir Arthur Eddington.

Dr. C. H. Cleminshaw has been promoted to Associate Director of the Griffith Observatory at Los Angeles in recognition of more than four years of service as acting director during the absence of the Director on war service.

Dr. Harlow Shapley, director of the Harvard College Observatory, was, on December 29, elected president of the American Association for the Advancement of Science for the coming year. Dr. Edmund W. Sinnolt of Yale University was named president-elect and will assume the duties of president in 1948.

The Rittenhouse Astronomical Society of Philadelphia held its monthly meeting on January 10, 1947, in the Morgan Physics Laboratory, University of Pennsylvania. The evening program consisted of the installation of new officers, and the address of retiring president, Dr. Louis C. Green, on the subject "The Light of the Sun."

The Portland Amateur Telescope Makers and Observers, according to the president's report, has had a busy and an interesting year in 1946. A number of the members have made or are making reflecting telescopes. Various members also have made observations of occultations, variable stars, sunspots, and meteors. Mr. T. P. Maher, Arlington, Oregon, is the president.

Norton's Star Atlas. *Corrigendum.* In the 6th and following editions of this work, including some early copies of the 10th edition recently published, the definition of the position angle of double stars is incorrect. On page 5, line 8 of the "Atlas," for "clockwise" read "anti-lockwise" and delete the words "or in the opposite direction to R.A." The position-angle diagram on page 39 is correct.

AUTHOR.

British Astronomical Association Circulars,—Number 270 of this series gives the information that it has been decided to extend the scope of the *Circulars* so as to give not only information of discoveries of comets, novae, etc., but other news, especially items of topical interest or of immediate importance to observers such as developments in spots on the surfaces of Jupiter or Saturn, exceptional solar disturbances, significant changes in variable stars, etc. This number is a 4-page and contains items on variable stars, Saturn, three comets, and an aurora. The *Circulars* are issued at University Observatory, Oxford, under the direction of Dr. G. Merton.

Eclipse News to be Broadcast

The National Broadcasting Company will participate in the National Geographic Society-Army Air Forces expedition to Brazil to observe the total eclipse of the sun next May 20. NBC will develop news broadcasts and television programs of the celestial show.

NBC will send a commentator, a crew of radio engineers, and television motion picture personnel with the expedition, which is being directed by Dr. Lyman J. Briggs, chairman of the National Geographic Society's research committee. A score of scientists will set up a portable laboratory near the town of Bocayuva, about 400 miles north of Rio de Janeiro.

Broadcasts will be made before, during, and after the eclipse, which will start about 7:35 A.M., Eastern Standard Time. The period of totality will be about four minutes. As film stories become available during the expedition, they will be flown to the United States for showing over NBC television outlets.

The Cleveland Astronomical Society

Dr. G. V. van Albada of Holland, Research Fellow at the Warner and Swasey Observatory, addressed the meeting of the Cleveland Astronomical Society on January 10. His topic was "Astronomy in Europe During the War."

Dr. van Albada pointed out how difficult it was to talk on astronomy in Europe during the war because communications were so bad that it was not even possible to keep posted on what was taking place only a few miles away. He said the European astronomer goes at his work in a way different from that of the American astronomer. Europe does not have the large and expensive instruments found in America. The largest telescope in Holland has an aperture of 13 inches. For observations or data obtainable only with the larger instruments they are dependent upon plates or data sent over to them. Some astronomers come to America to take these observations. Others limit themselves to theoretical work. Solar work is more advanced because the instruments necessary are not so expensive.

Plates sent to Europe are used to the utmost. More information is obtained

from them there as they are studied more thoroughly. The instruments used in obtaining the plates are used to the utmost here and there is not time to keep up with the observational work.

From Dr. van Albada's description of the conditions which existed in Europe during the war it is hard to realize how astronomers had any time or inclination to think of astronomy. In spite of the constant threat of famine or destruction a great deal was accomplished as was outlined by the speaker. New methods and instruments of research were developed. Valuable work on the sun, planets, stellar spectra, stellar interiors, stellar distribution, galactic structure, and absorption matter in space was done. Two new theories for the origin of the planets were advanced. These are only some of the things accomplished by astronomers in Europe under great difficulties. Dr. van Albada's lecture was interesting and inspiring.

HENRY F. DONNER.

Western Reserve University, Cleveland 6, Ohio.

Summary of Sun-Spot Observations at Mount Holyoke College, 1946

Month	No. of Obs.	North of Equator No. of Groups	North of Equator Av. Lat.	South of Equator No. of Groups	South of Equator Av. Lat.	Av. No. Groups at one Obs.	New Groups
January	20	10	+21°8	9	—18°5	4.0	19
February	18	15	23.1	14	18.2	6.1	27
March	27	14	23.3	21	20.6	6.1	27
April	23	6	25.7	24	21.0	5.6	25
May	20	17	23.4	14	22.0	5.9	22
June	24	20	22.0	20	20.2	5.9	37
July	27	16	20.4	14	21.1	5.4	24
August	22	16	18.3	11	15.7	5.2	24
September	25	21	16.9	17	19.5	6.6	31
October	27	19	15.3	22	15.6	8.3	36
November	21	21	17.2	18	16.8	8.8	33
December	22	24	+20.2	20	—16.1	8.8	40
Totals	276	198		203			345

Average number of groups at one observation.....6.42
Average latitude of 172 groups north of the equator.....+19°95
Average latitude of 173 groups south of the equator.....—18°92

As judged by the number of groups recorded during the year, the two solar hemispheres have averaged alike in activity. The greatest disparity occurred in April when only 6 of the 30 groups seen appeared north of the equator. On no day was the Sun free of spots. On 30 occasions during the last four months of the year more than 10 groups were simultaneously visible. A maximum of 13 was recorded on Oct. 19 and Nov. 18.

The record for 1946 parallels closely that of 1936 in average latitude of spots seen, with the average number at one observation running 10 per cent higher. One may infer that the approaching maximum will again be very high. The spread in solar latitude in 1946 is great. Noted at 35° or higher were 15 groups (3 N, 12 S); within 10° of the solar equator, 24 groups (9 N, 15 S). Extremes of latitude recorded were: —42° for a group of five spots seen Sept. 28-29, and +5° for a single spot first seen Sept. 16 near the east limb, and followed all the way across the Sun's disk.

New to our annals is the showing of as many as 27 observations in each of three months (March, July, October). The banner total of observations on 276 days is due partly to the favorable weather, partly to the industry of a group of helpers in South Hadley (Miss Harrison, Miss Jacoby, Mrs. Ryan, Miss Snow, Miss Tinker) who took over the daily recording of spot numbers and positions during much of the summer vacation.

Most of the records have been made by Miss Helen Pillans. The total number of individual *spots* counted was 7763.

ALICE H. FARNSWORTH.

John Payson Williston Observatory, South Hadley, Mass., January, 1947.

A Possible Recent Meteorite

The following information recently given me in good faith by Mr. Wesley Tomscheck, an ex-G.I. member of one of my college Geology classes, if correct, is certainly worthy of more detailed investigation. The writer, therefore, would greatly appreciate correspondence from any reader either residing in this country or abroad who can throw further light upon the subject.

Professor Hans Gerd Kock, a distinguished linquist and former Professor of Geology in a prominent German University,* working on a P.O.W. detail building swimming pools for the American Army, during the summer of 1945, was employed as an interpreter.

In one area, located on the Champagne Plain one and one-half kilometers east of Marmolow, many small meteorites were encountered, which, when broken open with a sledge, revealed a bright metallic lustre which was observed by Sgt. Tomscheck, who was in charge of the building operations.

Professor Kock explained their presence, by saying that while moving through the area one night, when it was under German occupation, he saw a huge meteorite strike the ground in the distance, which when found was estimated to be as large as a "squad tent," or about six feet long and three feet high.

Shortly after the fall, the area was captured by the British and soon thereafter transferred to American hands. Kock, who being captured at the time, stated that this was the first time this information had been revealed. The story was verified by other P.O.W's working on the same detail.

BEN HUR WILSON.

Joliet Junior College, Joliet, Illinois.

*Name of university was not obtained.

In Early Winter

When Winter whistles a melody
To which the snowflakes dance and play
And oak leaves chant in ecstasy
And shadows march at close of day,

Orion, jewel-decked and bright,
Then slowly mounts the evening sky
While Sirius follows in brilliant light,
And Pegasus is flying high.

—LISA ODLAND.

2649 35th Ave. S.,
Minneapolis 6, Minnesota.

Book Review

What Industry Owes to Chemical Science. Based on a scheme by Richard B. Pilcher and Frank Butler-Jones with Fifty Contributors. Issued by Authority of the Council of the Royal Institute of Chemistry. (Brooklyn, New York: Chemical Publishing Co., 1946. pp. viii-372. $5.00.)

The volume under review is an outgrowth of an earlier edition which consisted of some twenty articles contributed to *The Engineer* during the years 1916-17. The purpose of this early publication was indicated in the Introduction by the late Sir George Beilby, F.R.S., as supplying an answer to the question which was being widely discussed in England at that time: "What is the place of the chemist in practical life and what part has he played in industrial and social development?" While these early articles were printed during the stress of the first World War, they nevertheless aroused great interest and a second edition was published which was out of print soon afterwards.

About fourteen years later in 1939, the publishers reported that they continued to receive many inquiries for this type of book with requests for a new edition. Since the field of industrial chemistry had made such enormous strides in that interval of time, the re-publication was planned on a much bigger scale and the result was a new book in which 53 authors present their respective contributions by way of a symposium.

The contents consist of eighteen chapters including the following: agriculture; food; water supplies and sanitation; pharmaceutical products; soaps, waxes, and glycerin; disinfectants, antiseptics and preservatives; technical and other chemicals; dyestuffs; textiles; pulp and paper; synthetic resins and plastics; India rubber; photography; coal and its products; heavy chemicals; minerals and metals; building materials, mortar, cement; transport, ship-building and dockyards.

The book is intended for the general reader as well as for students of chemistry. The authors supplied definite evidence in concrete form of the usefulness of chemistry in the growth of industry. There is hardly an industry which has not used to advantage its inventions and discoveries. Every chapter of the book is an exciting story of the contributions of chemical discoveries to the general problem of introducing new comforts and raising the standard of living.

This volume represents a tremendous contribution to adult education. It makes accessible in a readable and lively style to people in different walks of life the kind of data which they could never have known otherwise. The next step in this type of venture of translating the technicalities and intricacies of science in plain language is to show the social effects of the inventions and discoveries of industrial chemistry.

SAMUEL M. STRONG.

Carleton College.

POPULAR ASTRONOMY

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